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SHOULD THERE BE ACADEMIC PREREQUISITES FOR GRADUATE WORK IN PSYCHOLOGY?

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It is unwise to specify a list of courses to be required of every graduate student in psychology, for there is no question that the great diversity of the possible fields of specialization within psychology make it unreasonable to demand more than a small amount of uniformity in actual graduate training. Nevertheless, in spite of the breadth of the field, its basic facts and theories—even its methods—are not so diverse but that certain 'background subjects' remain pertinent throughout psychology. The medical schools, facing a similar situation, have at least partially solved the problem of premedical training. Yet, although the differences between the psychophysicist and the mental hygienist, for example, are no greater than those between the dermatologist and the obstetrician, there is no generally accepted 'pre-psychological' training.

The 1934 report of the Committee on the Ph.D. Degree in Psychology of the American Psychological Association [1] indicated no great amount of agreement among departments of psychology concerning prerequisites for graduate work in the field. Twelve of the 24 departments queried required no training in science other than psychology for admission to graduate work. The report indicates that most of the departments having no requirements would prefer that the applicant have some scientific training outside psychology, particularly in biology. Fourteen departments answered that "the prospective graduate student in psychology should be discouraged from taking large numbers of psychology courses at the sacrifice of

[1] PSYCHOL. BULL., 1934, 31, 67–72.

work in the basic sciences and in language." Eighteen departments thought that students specializing in such fields as educational, child, or social psychology should *not* be excused from training in the basic sciences or in experimental psychology.

These and other items in the report suggest that, in 1934, there was some agreement that training in the physical sciences, biology, and mathematics is desirable before admission to graduate work in psychology, but that there was little consistency among the departments in their actual requirements for admission. The present study seeks further empirical evidence concerning the advisability of establishing standard prerequisites for graduate work in the field.

Two possible lines of inquiry presented themselves. The first was to seek evidence for relationships between training and professional 'success,' to see whether there are differences in training between a group of admittedly 'successful' psychologists and a 'control' group matched with the former with respect to the length of time since the award of the Ph.D. The second was to sample anew and in greater detail 'public opinion' among psychologists concerning the matter of necessary or desirable prerequisites for graduate work. These 2 approaches were combined in a single questionnaire.

The questionnaire requested information concerning the psychologist's training in 9 college subjects: *college algebra, analytical geometry, calculus, statistics, physics, chemistry, biology, sociology,* and *anthropology.* In each instance the psychologist was asked to indicate whether he had acquired knowledge through: (1) formal undergraduate training, (2) formal graduate training, (3) independent study *before* the Ph.D., or (4) independent study *after* the Ph.D.

The criterion of 'knowledge of the subject' was established by the instructions: "For the fields among those listed below in which you have at some time acquired knowledge *equivalent to that ordinarily acquired in a year-course in college,* please check the category that indicates how and when your knowledge was acquired." Exceptions were made for analytical geometry and calculus, because these subjects are often combined in a single year-course. The criterion in these 2 cases was set at a half-year-course or its equivalent.

The respondents were also asked to indicate their attitudes concerning the desirability of *pre-graduate* training in each of the 9 subjects. In this part of the questionnaire the following categories were used: (1) the subject is a *necessary* prerequisite, (2) the subject is a *desirable* prerequisite, *but not absolutely necessary,* and (3) the subject is *neither necessary nor particularly desirable.* Space

was provided for the inclusion of other fields that might seem to the respondent to belong in either of the first 2 categories.

It is of course impossible to be other than arbitrary concerning the criteria for 'success' in any line of endeavor. In the present study the initial criterion was the fact that the individual had been 'starred' in the last (fifth) edition of *American Men of Science.*[2] However, this criterion yielded too few cases, and it excluded those who have achieved success relatively recently. A poll of the members of the department of psychology at Harvard University yielded another dozen individuals scattered over the country who, it was agreed, would be starred in the next edition of *Men of Science.* Membership on the Council of the American Psychological Association, or in the Society of Experimental Psychologists provided additional criteria. As a final check, the names obtained by these procedures were compared with a list of 'candidates for stars' in the forthcoming (sixth) edition of *American Men of Science.* In order to avoid the difficulty of making judgments with respect to the success of young psychologists, no one was included who had received the Ph.D. since 1930.

On the basis of these various criteria, a group of 60 'successful' psychologists was selected, and the questionnaire was sent to each of them. The control group was selected at random from the *Year Book* of the American Psychological Association. They were matched to the experimental group with respect to Ph.D. age; that is to say, if there were 7 individuals in the experimental group who had obtained Ph.D.'s between 1910 and 1914, then 7 other individuals of the same Ph.D. age were selected at random from the *Year Book.*

An examination of the returns indicates that there were no significant differences between the older and the younger psychologists either in training or in the attitudes that were sampled. Nor were there significant differences between the 'successful' psychologists selected on the basis of a star in *American Men of Science* and those selected on the basis of the other criteria of success mentioned above.

The data to be reported have been gathered from the 41 'successful' psychologists who returned the questionnaire, and from 41 members of the control group, matched (in five-year intervals) as to Ph.D. age. The 2 groups were not deliberately matched with respect to any other variables.

[2] In order to obtain a reasonably homogeneous group, only those psychologists who had received the Ph.D. from an American educational institution since 1905 were included.

Each respondent was asked to indicate in what field (or fields) of psychology his own work is being carried on. Table I summarizes the data with respect to this question. There were no significant differences between the control group and the experimental group with respect to fields of work, although there was a slight preponderance (4) of educational psychologists among the control group.

TABLE I

DISTRIBUTION OF FIELDS OF WORK AMONG 82 PSYCHOLOGISTS

Field	Number of Times Mentioned	Number of Times Mentioned First
General Experimental	48	35
Educational	24	16
Physiological	9	8
Vocational and Applied (Personnel, Industrial, etc.)	9	4
Genetic	8	4
Statistics (Psychometrics, etc.)	7	3
Social	7	3
Animal	7	3
Abnormal	7	2
Clinical	6	3
Personality	4	0

Table II summarizes the data for the 2 groups with respect to training. While no single difference in the table is as large as 3 times its standard error, the number of individuals in each group

TABLE II

COMPARISON OF THE TRAINING OF TWO GROUPS OF PSYCHOLOGISTS: (I) A 'SUCCESSFUL' GROUP (SEE TEXT FOR EXPLANATION), AND (II) A 'CONTROL' GROUP MATCHED FOR PH.D. AGE

(N= 41 for Each Group. All Figures Are in Per Cent)

	Undergraduate Training		Graduate Training		Independent Study		No Formal Training	
	Group I	Group II	Group I	Group II	Group I	Group II	Group I	Group II
College Algebra	85	83	0	2	2	2	15	15
Analytical Geometry	68	59	2	2	10	5	30	39
Calculus	39	34	9	7	17	5	52	59
Statistics	24	17	37	44	32	34	39	39
Physics	83	73	0	2	5	5	17	25
Chemistry	73	80	2	0	2	2	25	20
Biology	68	83	17	2	7	12	15	15
Sociology	44	39	10	9	10[3]	32[3]	46	51
Anthropology	7	10	15	12	27	41	78	78

[3] The difference between these 2 figures is the most nearly statistically significant in the Table. The difference is 2.5 times as large as the standard error of the difference. No other critical ratio is greater than 1.6.

is so small that differences would have to be huge to be statistically significant. There *is* an observable trend of difference between the 2 groups. The 'successful' group seems to have had slightly more formal training in mathematics, physics, and sociology than the control group. In fact there is only one instance (chemistry) where the frequency of formal training is greater for the control group.

It would be indeed surprising if one were to find a sizable correlation between amount of training in any subject and success in a profession. The determinants of success are legion, and amount of formal training is probably one of the less important of them. However, *given the other conditions for success,* the proper training becomes definitely important. The 'insights' of genius are no less dependent upon knowledge than are the logical conclusions from empirically established premises. In fact, it is the broad background of knowledge that often makes possible the perception of significant relations that are hailed as insightful discoveries.

The value of training in mathematics, physics, and perhaps chemistry lies less in its application to specific problems in psychology, and more in the rigor of the habits of thought that it instills. Knowledge of mathematics and of the 'exact' sciences provides tools for thinking—tools whose usefulness is great in any field of psychology. And the 'successful' psychologist possesses these tools more often than his 'average' colleague. Without them, regardless of the other factors that contribute to success, he would be distinctly handicapped. The data in Table II suggest that, although other factors are undoubtedly of greater importance, training in mathematics and physics is not unrelated to 'success.'

But the data in Table II provide little basis for stipulating prerequisites for graduate work in psychology. However convincing the logic of the previous paragraph, it does not provide the kind of empirical evidence in terms of which standards for admission to graduate school could be fairly established. Thus we may turn to 'public opinion' among psychologists. If they are in agreement concerning the value of specific pre-graduate training for psychologists, their judgment may be accepted as significant for they have, after all, been 'through the mill.'

Table III summarizes the attitudes of the 82 psychologists who contributed to this study. The differences between the opinions of the 2 groups were in general small, and, since there seemed to be no clear explanation for such differences as did appear, the data are combined in Table III.

TABLE III

ATTITUDES OF 82 PSYCHOLOGISTS CONCERNING PREREQUISITES FOR GRADUATE STUDY IN PSYCHOLOGY

(All Figures Are in Per Cent)

Course	"Necessary"	"Desirable But Not Necessary"	Column 1 Plus Column 2	"Neither Desirable Nor Necessary"
College Algebra	54	42	96	4
Analytical Geometry	20	51	71	16
Calculus	15	64	79	11
Statistics	77	23	100	0
Physics	33	60	93	1
Chemistry	16	57	73	16
Biology	76	24	100	0
Sociology	18	56	74	15
Anthropology	12	62	74	16
Neurology, Physiology	12	14	26	..[4]
Philosophy	5	12	17	..[4]

The majority agree that college algebra, statistics, and biology are *necessary* prerequisites. More significant, perhaps, is the fact that *70% or more feel that every one of the 9 subjects is a desirable prerequisite.* And they are more than 90% in agreement concerning college algebra, statistics, physics, and biology. Surely, if trained psychologists are so nearly unanimous in asserting the desirability of undergraduate training in these 4 subjects, the undergraduate 'major' in psychology would be wise to give heed to their judgment.

TABLE IV

COMPARISON OF ATTITUDES OF PSYCHOLOGISTS IN DIFFERENT FIELDS TOWARD THOSE PREREQUISITES FOR GRADUATE STUDY IN PSYCHOLOGY THAT ARE *Necessary*

(All Figures Are in Per Cent)

Course	General Experimental N=35	Educational N=16	Physiological Animal N=11	All Others[5] N=20
College Algebra	57	31	64	60
Analytical Geometry	23	6	45	10
Calculus	17	6	27	10
Statistics	69	88	91	75
Physics	43	13	36	30
Chemistry	9	0	36	30
Biology	71	75	100	70
Sociology	9	31	18	25
Anthropology	9	13	9	20
Neurology, Physiology	20	0	9	10
Philosophy	6	0	0	10

[4] No opportunity was given to check this category since these courses were not included in the questionnaire, but were added by the respondents.

[5] See Table I for the fields represented in this category.

The point is often made that the demands of the different specialized branches of psychology are so different that general prerequisites for graduate study cannot be specified. It has already been suggested that this may not be true, and the field of medicine has been cited as a comparable example where the specification *is* made. Tables IV and V seem to indicate that, even for psychology, this point has no real foundation in fact. While the educational psychologists, for example, differ in certain respects from the physiological psychologists concerning those prerequisites that are *necessary,* the various groups agree remarkably well concerning the general desirability of all the 9 subjects included in the questionnaire. One hundred per cent of the physiological psychologists feel that statistics, physics, chemistry, and biology are desirable prerequisites for graduate work, and the majority of each of the other groups is in agreement with this judgment.

TABLE V

COMPARISON OF ATTITUDES OF PSYCHOLOGISTS IN DIFFERENT FIELDS TOWARD THOSE PREREQUISITES FOR GRADUATE STUDY IN PSYCHOLOGY THAT ARE *Either Necessary or Desirable*

(All Figures Are in Per Cent)

Course	General Experimental N=35	Educational N=16	Physiological Animal N=11	All Others [6] N=20
College Algebra.....	91	88	82	95
Analytical Geometry..	74	63	82	65
Calculus.....	80	75	82	80
Statistics	100	100	100	100
Physics	100	75	100	90
Chemistry.....	66	56	100	85
Biology	100	100	100	100
Sociology.....	74	94	64	65
Anthropology	74	75	45	90
Neurology, Physiology	31	6	27	25
Philosophy	23	12	0	20

There was a certain amount of definite disagreement concerning the advisability of sociology and anthropology. Approximately 15% of the entire group thought that these subjects were *neither* necessary nor desirable, and one or two added that they were potentially harmful because "they required the expenditure of time that would be better devoted to other things." This attitude reflects the superciliousness of some psychologists toward those fields that have in the past been considered 'less scientific' than psychology. Until recently there was an identical attitude prevalent among physicists

[6] See Table I for the fields included in this category.

and chemists toward the biological sciences. Today, as the boundaries between the sciences become more and more hazy, the 'caste system' that has prevailed among the various fields of scientific endeavor is disintegrating.

The social psychologist was the first to recognize the importance of sociological and anthropological findings for his own understanding of the phenomena in which he was interested. His colleagues in other fields of psychology have not, in general, been slow to appreciate that, although the study of the human organism can be conducted from many points of view, the findings resulting from the different approaches are by no means mutually exclusive. The majority of the group involved in the present study are agreed that anthropology and sociology are desirable prerequisites for the graduate student in psychology. It is doubtful whether a majority agreement would have been obtained on this matter even so short a time as 5 years ago.

It is perhaps significant that, although the respondents were asked to add to the list of 9 subjects any others that they felt were either necessary or desirable, there were only 2 subjects each of which was added by more than 10% of the entire group.[7] The addition of neurology, neural anatomy, physiology of the nervous system, etc., was to be expected. These subjects were not included in the questionnaire because it was felt that the *undergraduate* could not be expected to do more than attain familiarity with the field of biology in general.[8] The fact that relatively few psychologists added these specialized subjects suggests that this assumption was tacitly accepted as correct by the majority.

Only 17% indicated that they felt training in philosophy to be either desirable or necessary. As the psychologist becomes more and more dependent upon scientific exactitude in establishing the facts of his field, he will undoubtedly find it less important to fortify his theories with philosophical erudition. There are those who argue that the separation of scientific psychology from philosophy is but temporary, yet the data of Tables IV and V indicate that there are relatively few psychologists today who feel that previous training in philosophy is important for the student entering the graduate school.

In general it would seem that psychology is ready to consider the

[7] Languages (particularly French and German) provided an exception to this statement, but they are so universally required today that they can safely be omitted from this discussion.

[8] This limitation is widely recognized by the medical schools in establishing prerequisites for medical work.

establishment of prerequisites for admission to graduate study. 'Public opinion' among psychologists is 'crystallizing.' Not only are there indications that the 'successful' psychologist tends to be trained in mathematics and physics more often than the average psychologist, but members of the profession, whether they are clinicians, applied psychologists, or experimentalists of the 'normal human adult' kind, are in quite definite agreement concerning the desirability of adequate scientific and mathematical training at the undergraduate level.

Legislation could not, of course, be recommended on the basis of the scanty evidence provided by this study. Nevertheless, the data are sufficiently univocal to suggest that the candidates' training in mathematics and in the 'exact' sciences, as well as in the social sciences, might profitably be considered before granting scholarships and assistantships at the graduate level. This study provides evidence that the diversity among the fields of psychology is no obstacle to the specification of reasonable and uniform prerequisites for graduate work, nor should it be difficult to obtain sufficient evidence to decide definitely what these prerequisites should be.

PROCEEDINGS OF THE EIGHTH SPRING MEETING, EASTERN BRANCH, AMERICAN PSYCHOLOGICAL ASSOCIATION

HERBERT W. ROGERS, SECRETARY-TREASURER, LAFAYETTE COLLEGE

The Eighth Annual Spring Meeting of the Eastern Branch of the American Psychological Association was held on Saturday, April 3, 1937, at Vassar College, Poughkeepsie, New York. It was attended by 360 persons; 196 being members or associates and 164 guests.

The scientific program consisted of 61 papers arranged in 9 sections: Conditioned Responses, Sensory Processes, Abnormal Psychology, Attitudes, Learning and Memory, Brain Processes, Comparative Psychology, Psychometrics, and Applied Psychology.

Elections. President, 1937–1938, K. S. Lashley, Harvard University; *Board of Directors,* K. M. Dallenbach, Cornell University, 1937–1940, and J. W. Dunlap, Fordham University, 1937–1940; *Secretary-Treasurer,* H. S. Oberly, University of Pennsylvania, 1937–1940.

Committees Appointed. Program: G. W. Hartmann, Teachers College, Columbia University, Chairman; T. W. Forbes, Harvard University; Edna Heidbreder, Wellesley College. *Nominating:* Theodora M. Abel, Trade Extension Classes, New York City, Chairman; Irving Lorge, Teachers College, Columbia University; George Kreezer, Vineland Training School.

The growth of the Branch made necessary several changes in the By-Laws. These changes, passed at the Business Meeting, were as follows:

Article I, Membership, Section 1, now reads: "The Eastern Branch, affiliate of the American Psychological Association, shall consist of persons who are Members or Associates of the American Psychological Association in good standing and who are located in the Eastern part of the United States or Canada." The result of this change is to broaden the Branch territory.

Article II, Meetings, Section 1, now reads: "The Annual Meeting shall be held on the first weekend in April, if it is a convenient date." This specifies the meeting date, tends to avoid conflicts with other meetings and the Easter Holidays.

Article II, Meetings, Section 2, changed from "Other meetings of the Branch shall be held at such time and place as shall be decided by the Board of Directors and the Program Committee" to read: "Other meetings of the Eastern Branch shall be held at such time and place as shall be decided by the Board of Directors."

Article IV, Officers, Section 1, changed from "The officers of the Branch shall be: an Honorary President and a Secretary-Treasurer" to read "The officers of the Branch shall be: a President and a Secretary-Treasurer." This change will make the office a more active one.

Article IV, Officers, Section 5, an addition to read: "The Secretary-Treasurer shall be adequately bonded."

Article VI, Committees, Section 5, provides for the establishment of a new committee as follows: "The President shall appoint two members of the Board of Directors as an Auditing Committee. The duty of the Auditing Committee shall be to audit the accounts of the Branch at the end of each fiscal year, May 31, and submit a report to the Directors not later than June 10."

Members of the Branch voted to express to Professor M. F. Washburn their sympathy and regret upon her illness and absence from the meeting.

Next Meeting. The invitation to hold the next Annual Spring Meeting at New York University, University Heights, New York City, on April 2, 1938, was accepted.

Following the luncheon and preceding the Annual Business Meeting, the retiring President, Samuel W. Fernberger, delivered the Presidential Address on *A Psychological Cycle.*

Abstracts of the papers presented follow:

Conditioned Responses

Chairman, Howard S. Liddell, Cornell University

Sensory Irradiation with Voluntary Responses. Eleanor J. Gibson, Smith College.

This experiment was designed to test the proposition that an irradiation or generalization gradient such as is found in the typical conditioned reflex situation will occur also when the response is a voluntary verbal one rather than a reflex. The reaction time experiment provided the situation for obtaining a voluntary response. Four vibrators were employed in a series across the subject's back in one group, and up and down the subject's back in another. The subject was instructed to respond by speaking a nonsense syllable to one of these but to no others. For half the subjects the "trained" position

was at one end of the series, and for half at the other end. Reaction time was measured by means of a voice key and Dunlap chronoscope. Fifty practice trials were given in reacting to the " trained " stimulus, after which the other 3 vibrators were interspersed at random intervals among trials with the " trained " stimulus. Degree of generalization was indicated by the number of responses made to the 3 prohibited stimuli.

Results with a series of vibrators extending vertically up and down the back showed the typical irradiation gradient, frequency of response varying with proximity of the stimulus to the trained one. A horizontal series of stimuli gave a similar gradient, except that the stimulus symmetrical with the trained point showed a rise in the curve. Frequency of response was not equivalent, however, to that at the trained position (as Anrep reported). Reaction times to the prohibited stimuli are analyzed.

The Rate of Experimental Extinction as a Function of the Interval Between Successive Non-reinforced Stimulations. J. M. PORTER, JR., King College, Bristol, Tennessee.

A logical implication of the Pavlovian theory of extinctive inhibition is to the effect that the number of non-reinforced presentations of the conditioned stimulus necessary to extinguish a conditioned response is a function, in part, of the length of the interval of time permitted to elapse between successive stimulations. Inspection of the data which have been reported in this connection shows that they do not confirm this inference. Such data are fragmentary and do not afford a decisive answer to the problem, but the apparent indication is that the number of non-reinforced stimulations required to secure extinction is independent of the length of inter-stimulus interval.

In the investigation to be reported conditioned eyelid responses were established in 40 adult human subjects under approximately identical conditions. Each subject was then assigned, in a random manner, to 1 of 5 " extinction groups." The interval between successive presentations of the non-reinforced conditioned stimulus was different for each of the groups of subjects.

The data indicate no significant difference with respect to the number of non-reinforced stimulations required to secure extinction of the response when the interval between successive stimuli was varied between 10, 20, 40, 80, and 180 seconds. Furthermore, within the limits of this investigation, the rate at which certain characteristics of the conditioned response are modified during successive por-

tions of the extinction process, as well as the extent to which they are modified before extinction occurs, is unrelated to the length of the inter-stimulus interval. It is suggested that these facts are not inconsistent with the view of experimental extinction as a learning process, but that they are definitely at variance with certain predictions which logically follow from the Pavlovian theory of extinctive inhibition.

Generalization of Extinction in a Two-Segment Serial Habit. Douglas G. Ellson, Yale University.

Nine dogs were taught to obtain small rubber balls from a vending machine and deposit them in a second vender which then automatically delivered a pellet of food. The 2 machines were placed in separate compartments of a cage. On each experimental day the dogs were subjected to 1 of 3 conditions for a fifteen-minute period: (a) the food habit was extinguished, balls being obtained from the ball vender; (b) similar to (a) except that balls were obtained from a pan on the floor in place of the ball-vender; and (c) the dog was placed in the cage with neither machine present, so that presumably no extinction could occur. Three minutes later the dogs were returned to the cage and the ball habit was extinguished during a second fifteen-minute period. Each dog was subjected twice to all 3 conditions.

The mean number of extinction trials on the ball habit during the second fifteen-minute periods were as follows:

Condition	No. Extinction Trials of Ball Habit	Per Cent Decrease from Control Conditions
(a) Ball Habit in Series During Extinction	11.4	48%
(b) Ball Habit Not in Series During Extinction	17.6	21%
(c) Control: No Extinction	21.9	0%

Fifty trials in which both segments of the habit were reinforced were given on the days between each experimental day (extinctions). Between the third and fourth experimental day 300 reinforced trials were given, 50 per day for 6 days. In spite of this additional reinforcement following the preceding extinction, the number of trials required to extinguish the ball habit showed a steady negatively accelerated decrease as follows:

Experimental Day (All Conditions)	1	2	3	4	5	6
No. Extinctions on Ball Habit	37	21	13	13	11	7

Higher Order Conditioning in Cats with the Motor Alimentary Conditioning Method. C. N. WINSLOW, New York State Psychiatric Institute, R. KANTOR, Columbia University, and C. J. WARDEN, Columbia University.

In view of the fact that Finch and Culler demonstrated that higher order conditioned responses could be obtained with dogs well beyond the third order, which is claimed as the limit of higher order conditioning in these animals by Pavlov, we were interested in determining whether higher orders were not possible in cats beyond the third.

The experiment was performed at the New York State Psychiatric Institute and Hospital. The apparatus consisted of a box made partially sound proof by a one-inch layer of insulating fiber board on the inside walls. Within the box was a feeder and the devices for producing the various stimuli. The method employed with minor modifications was the motor alimentary conditioning method devised by Dworkin, in which the securing of food serves as the unconditioned stimulus, and raising the lid of the feeder as the response.

The conditioned stimuli of the various orders were introduced in turn. They were bell, red light, buzzer, light illuminating the box extinguished, and a blast of air. Several different sequences were introduced with different animals.

Of the 5 animals used, 4 were carried through the fifth order, the same level as that attained in dogs by Finch and Culler. We found as had these investigators that the speed of conditioning is accelerated as the animal is taken from level to level of conditioning, and not retarded. We would also agree with them that conditioning apparently can be carried on almost indefinitely to higher orders, provided that sufficient stimuli in the various sense fields can be found. The cats' behavior in responses became more stereotyped as the higher orders were reached, and lost the heterogeneity that characterized it in the early stages of conditioning.

Retention of a Potential Trial and Error Response in the Rat. RICHARD E. P. YOUTZ, Yale University.

In Pavlovian conditioning the indifferent stimulus acquires the ability to evoke the response to the reinforcing stimulus. In Thorndike's trial and error learning, a chance response to a stimulus is followed by a reinforcing situation (reward). The indifferent stimulus then evokes this formerly chance response, and not, as in

conditioning, the response to the reinforcing stimulus. If these 2 are types of conditioning, as several experimenters suggest, the trial and error response will not lose strength through disuse, but will, like the conditioned response, maintain or even increase its strength over short periods of time.

Twenty rats of one group were given 40 reinforced (rewarded) trials on a bar-depressing response of the type found in trial and error learning. Another similar group were given only 10. The strength of this bar-depressing tendency could be measured by its resistance to extinction (during which no reward followed the response to the bar). Twenty-four hours after training, the strength of this tendency was tested in half of each group, while in the other half it was measured after 15 days. In order to test for spontaneous recovery, all of the rats were given a second extinction 24 hours after the first.

Both the ten-reinforcement and the forty-reinforcement groups gave a greater number of extinction-responses at 15 days after training than at 24 hours, showing an apparent increase in strength of the tendency through disuse. At both 24 hours and 15 days the forty-reinforcement tendencies were stronger than the 10. All differences were in the same direction but were much reduced after spontaneous recovery. It is suggested that Hilgard and Campbell's proposed explanation of similar phenomena in the retention of conditioning applies here.

Configural and Colligated Conditioning. G. H. S. RAZRAN, Long Island University and Columbia University.

Configural conditioning is used here to denote conditioning to a pattern of stimuli when only the pattern but not the components elicits the C-R, while colligated conditioning is used when both pattern and components are effective. In the reported experiment, with 12 college students divided into 3 groups, both types of conditioning and their interrelations were studied by combining the consumption of food with the flashing of differently colored electric bulbs and testing the salivary C-R's to both pattern and components, at different stages of training and varying attitudinal settings. Group A consumed their food while solving a visual maze in which the flashings of the bulbs signalled right or wrong pathways. Group B ate, observing group A solve the maze, while the conditioning with group C was of the usual laboratory set up, with no maze involved. The results were: (1) Configural conditioning appeared in all 3

groups, differing markedly from colligated—and orthodox conditioning in general—in that, with training, it became less dependent upon the particular colors of the bulbs. (2) With the exception of group C, configural conditioning was preceded by a colligated stage of conditioning. (3) Configural conditioning appeared earlier when the attitudinal setting was perceptual, as in group B, than when it was motor, as in group A. The conclusions are: (1) Configural conditioning differs *qualitatively* from colligated and simple conditioning. (2) Configural conditioning is an *emergent* rather than a *prius*. (3) Configural conditioning is a dynamic function of the individual at work rather than a 'spontaneous and compulsory grouping of data in an organized environment.'

The Startle Pattern in Children and Identical Twins. WILLIAM A. HUNT and FRANCES M. CLARKE, Connecticut College for Women.

An investigation of the behavior of 34 male and female children, ranging in age from 3 to 14 years, showed the presence of the startle pattern in response to a revolver shot. Both the primary pattern and the subsequent secondary movement were greater than in adults. The pattern is found in the negro race. Included among the children were 10 pairs of identical twins. In every case the members of a pair of identical twins were found to show differences in the amount of response to the shot. There is a tendency for the greater startle pattern to be shown by the left-handed member of the pair.

SENSORY PROCESSES

CHAIRMAN, HERBERT S. LANGFELD, PRINCETON UNIVERSITY

Thermal Adaptation in Relation to the Dimensions of the Stimulus. W. L. JENKINS, Lehigh University.

In a previous report by the author, it was shown that complete adaptation to a temperature of 10° C. occurred in a few seconds with a stimulator 1.25 mm. in diameter surrounded by a field maintained at skin-neutral. The same technique has now been extended to include a series of circular stimulators up to 13 mm. in diameter with temperatures of 10° and 44° C. While time for complete adaptation increases definitely with increase in stimulus size, the increase is *not* a matter of simple proportion to the area of the stimulator. In fact, early results in a systematic series with rectangular and annular stimulators indicate that area as such (especially in the smaller sizes) may be of relatively minor importance in determining adaptation time. The parts played by such factors as

perimeter and least dimension and the possibility of thermal contrast are tentatively discussed.

The Curve of Intensity Discrimination in the Cat as a Function of Illumination. LEONARD C. MEAD, University of Rochester. (Introduced by W. E. Kappauf.)

Thresholds of visual intensity discrimination in the cat were obtained at 4 different levels of illumination. The animals discriminated between 2 test patches 10.2 cm. on each side, the centers of which were separated by a distance of 38 cm. The response of the cat consisted in depressing a small lever in accordance with the position of the more intense stimulus. An electric shock was administered for an incorrect response. The threshold values obtained were based upon a failure of the animal to discriminate correctly at least 75% of the time throughout at least 2 daily trial periods. The different subjects used received a total of either 20 or 25 trials per day. Intensity variations were secured by means of neutral tint filters.

In the experiment, threshold determinations were carried out on 3 animals at a level of illumination of 280 millilamberts, on 5 animals at 50.4 millilamberts, and on 3 animals at 0.48 millilamberts. Preliminary data are available on 3 animals at an illumination level of .023 millilamberts. Additional observations are now being made at these and at other levels of illuminations.

The ranges of $\frac{\triangle I}{I}$ for the different animals at the different levels of illumination were as follows: 0.26–0.6 at 280 ml., 0.24–0.6 at 50.4 ml., and 0.23–0.6 at 0.48 ml. The average values of $\frac{\triangle I}{I}$ for the various animals is relatively constant over this range of illuminations. It is concluded that the method used makes possible a complete determination of the threshold of the intensity discrimination in the cat as a function of illumination. The results secured in the present study with reference to the visual capacity of the cat are compared to similar data on other animals.

Absolute Brightness Thresholds in the Cat. CHARLES S. BRIDGMAN, University of Rochester.

An apparatus employing features of the lever type discrimination situation and the method of conditioning was devised for testing absolute brightness thresholds and similar visual abilities in the cat and other typical laboratory mammals. This apparatus consists of a box, 125 cm. long, 32 cm. wide, and 43 cm. deep, separated into compart-

ments by a door which contains a flashed opal glass stimulus panel. This door can be opened by pressure on a lever located near the floor of the larger or response compartment.

In the experiment, cats were trained to press the lever and pass forward into the smaller compartment to receive food when a light stimulus was presented, and to refrain from touching the lever when no light was present. Incorrect responses resulted in the administration of a shock. Following successful responses the animals were returned to the response compartment, and after a variable length of time the stimulus was again presented.

Absolute brightness thresholds were determined by progressively reducing the stimulus intensity with neutral tint filters, until a point was reached at which dark-adapted animals were unable to react successfully, *i.e.* make a correct choice in 80% of the 20 trials administered daily.

Thresholds determined on 4 cats by this method are of the order of 10^{-7} millilamberts. The consistency of the results obtained and their relation to results of similar studies are taken as indicative of the reliability and validity of this method in threshold measurements.

The apparatus may be used in the investigation of low and high level visibility functions, and the course of dark-adaptation.

A Comparative Study of Optic Nystagmus. WILLIAM E. KAPPAUF, University of Rochester.

An apparatus which makes possible a constant or variable speed translation of the entire visual field has been developed for the investigation of special phenomena related to optic nystagmus in humans and in animals.

A moving pattern of equally spaced vertical striations is obtained by rotation of a drum which, being constructed of 50 circular ribs, is partly spherical in shape. The white cardboard stripes, mounted 1 on each rib, are uniformly illuminated from top to bottom by a frosted bulb located at the center of the drum. Since the light passing between stripes is absorbed rather well by the black room walls several feet distant, the white stripes (brightness—40 millilamberts) contrast sharply with the background.

During a series of observations, the subject's head is located within the drum just below the center. The drum is set in motion at a desired speed and the light turned on at intervals. Oscillograph records are taken of the potentials picked up from temporal electrodes before, during, and after each presentation of the light.

Preliminary data on nystagmus movements as a function of the speed of movement in the visual field are available for a group of 8 cats and 4 human subjects. At low speeds of rotation, the ocular pursuit is regular and rhythmical. As the drum speed is increased, the records are complicated by rapid gross movements of the head and by fast eye-movements in the direction of rotation. At still higher speeds, all movements of the head and eyes disappear. Critical speeds for both animal and human subjects are very variable.

Evidence of after optic nystagmus has been found in the records of all but 1 of the subjects.

The Visual Discrimination of Real Movement in the Cat. CLIFFORD T. MORGAN, University of Rochester. (Introduced by L. Carmichael.)

The question of the nature of visual movement discrimination in infrahuman mammals is of significance in the understanding of certain aspects of the neural mechanisms of vision. This paper describes a method especially designed for the study of the discrimination of moving striated patterns in the cat and, in addition, presents certain preliminary results bearing upon the capacity of this animal in responding to such patterns of visual stimuli.

The method used requires the animal to discriminate between a moving and non-moving pattern of striations by depressing a lever. The stimulus patterns, which consist of alternate black and white vertical lines 5.0 cm. wide, are presented by means of 2 modified motion picture projectors upon diffusing screens located in the doors of 2 large discrimination boxes. The apparatus and technique used permit rapid variation in the speed and position of the stimulus patterns, as well as the control of extraneous sensory influences.

To date, 3 animals have been trained in the discrimination of the moving striated patterns. These animals required approximately 200 trials in order to fulfill a criterion of 90% correct responses in 40 trials. Determinations of the limens of absolute movement discrimination in these animals will be discussed in relation to the validity of the present method in further investigation of the visual mechanism of the cat.

The Effects of Sodium Chloride upon the Electrical Responses of the Cochlea. ERNEST GLEN WEVER and CHARLES W. BRAY, Princeton University.

The course of changes in the electrical responses of the cochlea of the guinea pig were studied for different frequencies of sound

after application of sodium chloride to the round window. Typically, there was an initial augmentation, followed by an early rapid loss and then by a protracted decline of more moderate rate. The early loss occurred in about the same degree for all frequencies. The secondary loss was usually differentiated with respect to frequency, with high tones commonly suffering more than low tones. In some cases there were partial recoveries.

A study of the functional relations between intensity of stimulation and magnitude of responses showed little alteration in form or slope of the functions for high tones, and slight reductions of slope for low tones.

In experiments in which salt was applied sometimes to the round window, and sometimes to the apex, concurrent observations from round window and apex showed a difference in the effects as recorded from the 2 positions: the impairment of response was greater from the electrode in the region where the substance was applied.

The early, rapid loss, which involved all frequencies, is attributed to some general process such as a disturbance of pressure relations within the cochlea. The secondary decline, in which high tones usually were involved more severely than low tones, is consistent with a theory of progressive impairment of hair cells.

Three features are discussed in relation to the problem of cochlear localization. These are the low degree of specificity with regard to stimulus frequency, the differences in the observations at base and apex, and the greater reduction in slope of the intensity functions for low than for high tones. These facts suggest broad rather than specific localization of tones in the cochlea.

Adaptation to Vibratory Stimuli. S. B. CUMMINGS, JR., and C. H. WEDELL, Princeton University.

This experiment was devised to test statements in the literature, based on casual observation, that adaptation to vibration does not take place.

The output of a 2-A audiometer was conducted through a vacuum tube amplifier and voltage attenuator to a magnetic loud-speaker motor which had been modified so as to drive a small fiber rod. The rod was in contact with the fleshy part of the palm of the hand over an area approximately 1 mm. in diameter. Threshold determinations were made immediately after 3 minutes continuous stimulation, and at stated intervals thereafter until the threshold had returned to normal.

The results for the 6 subjects show that adaptation is greatest for 1,024~, less for 236~, and least for 64~, after stimulation for 3 minutes at 25 db. above threshold. Stimulation at 1 frequency affects all 3 frequencies equally. Stimulation with a frequency of 256~ at 45 db. above threshold affects the threshold more than stimulation 10 db. above. The amount of adaptation varies greatly from subject to subject.

The relation of these results to other work on vibratory sensitivity is indicated, and the theoretical implications are discussed.

The Upper Limit of Cutaneous Sensitivity to Frequency of Vibration in the White Rat. ALEC SKOLNICK, Princeton University. (Introduced by E. G. Wever.)

The experiment is a preliminary study of the upper limit of the sensitivity to vibration in the white rat.

The apparatus consisted of a starting base and 2 stimulus-platforms. A separate vibrator was connected to each of the stimulus-platforms, either of which could be set into vibration by an electric oscillator.

Four blinded animals were used. Two were trained to cross to the vibrating platform, and 2 to the non-vibrating platform.

The upper limit (point at which the animals responded correctly 75% of the time) was found to lie between 1,800 and 2,048 cycles. The results correspond closely with those obtained in human subjects when the same apparatus is used.

ABNORMAL PSYCHOLOGY

CHAIRMAN, H. L. HOLLINGWORTH, COLUMBIA UNIVERSITY

A Phobia Originating Before the Age of Three Cured with the Aid of Hypnotic Recall. G. MILTON SMITH, College of the City of New York.

This case is of considerable interest because of the early age (objectively verifiable) at which the causative trauma occurred, and because of the wealth of detail revived with the aid of hypnosis.

L. I., a youth of 19 years, had suffered for years from a fear of the darkness so intense that he was unable to remain alone in a dark room. He was also distressed by his inability to remember anything about his father, who had died when the boy was 5, and by his mother's persistent refusal to enlighten him. This reticence, it developed later, was due to the fact that the father had spent the last 2 years of his life in an asylum suffering from paresis.

When L. was asked under hypnosis if he could recall any terrifying experience associated with darkness, he described in astonishing detail the interior of a certain room, later identified with an apartment which the family had left 14 years before. In this room he saw a child (himself) and a man (his father), whom he described in some detail, which was subsequently verified. At a second session, he saw himself in this same room playing while his father sat reading. Without warning, his father jumped up and began to yell and wave his arms about; his face was horribly contorted. The child fled in terror down a long dark hallway, his father staggering after him.

During this recall L. experienced considerable emotion; his phobia was intensified that night. Some days later, shortly after a third session, the symptoms disappeared. There has been no relapse for over 7 months. The case seems to fit neatly into the etiological pattern suggested by Bagby: traumatic fear experience followed by shame-motivated repression.

A Study of Mentally Defective Subjects in a Free Situation. ELAINE F. KINDER, Letchworth Village.

A standardized observation-room technique, including a constant random sampling of play objects, was employed in the investigation of high grade, clinically undifferentiated, mentally defective subjects of different chronological ages, mental ages, and intelligence quotients; and of low grade subjects of the mongolian and microcephalic types. The distribution of interest and type and duration of responses in the various groups have been studied with a view to determining chronological and mental age differences. Repetition of the experiment after successive six-month intervals showed a high degree of consistency of behavior for individual subjects.

The Perseverating Tendency in Stutterers. JON EISENSON and CHARLES N. WINSLOW, Brooklyn College.

Results of 2 experiments will be presented showing that stutterers perseverate in non-speech activities. One experiment is of the paper and pencil type composed along these lines:

1. A situation is presented to which the subject is required to respond for a given time period.

2. A variation of this situation is then presented to which the subject is required to make a different response for the same time period.

3. A third situation, consisting of the combination in random order of both original situations, is then presented.

The second experiment involves perception of colors and geometric forms presented by a Whipple tachistoscope.

The results of the first experiment, already completed, and the second, about to be completed, indicate that stutterers perseverate to a greater degree than non-stutterers. It is concluded that stutterers, as personalities, are perseverators, and that the perseverating tendency is revealed in their speech in repetition of sounds, or in a blocking of sounds which prevents free flowing rhythmic speech.

The Effect of Initial Capacity on Deterioration. HAROLD M. HILDRETH, Syracuse Psychopathic Hospital.

This paper reports briefly the results of 1 of a series of studies in which the commoner types of cerebral pathology are used for experimental study of cortical function.

The study reported investigates the extent to which initial intellectual capacity determines the degree and character of ensuing deterioration of intellective functions.

Four experimental groups, each representing a specific pathological change in the cortex, are compared with a control group as to performance in a given series of tests. Errors of difference are computed throughout, giving a deterioration measure for each test. These values ranked give a pattern for each group. Each of the 4 experimental groups is then divided into 4 sub-groups on the basis of initial intellectual level.

Results show that initial intellectual capacity influences both degree and pattern of subsequent changes, and that this differential deterioration is independent of the type of pathology to a significant degree.

Rorschach Personality Studies Before and After Operation for Brain Tumor. AARON B. NADEL, Columbia University and Montefiore Hospital, New York.

Much has been learned about the personality of individuals suffering from organic diseases of the brain. Investigations have been carried out on the personality set up of organic brain disease, in general, and of cases with frontal lobe involvement. However, it is of interest to study the framework of the personality of cases with verified brain tumor, that is, verified by operation. Opportunity was afforded to study a number of cases before and after operation.

The following case, selected as a demonstration case, serves the purpose of showing whether and how personality changes connected

with the operation can be shown by the Rorschach technique. In the results of this investigation, 2 points are of particular value:

(1) The state immediately before the operation shows a dominance of the physical condition. The individual personality picture is still visible but somewhat blurred by the obsession and preoccupation with the existing physical condition.

(2) After the operation, this preoccupation vanishes and the individual personality patterns appear much more definite in shape.

Cultural Influences in Psychopathology. JAMES PAGE and CARNEY LANDIS, Columbia University.

Recently various anthropologists have stressed the influence of the total culture of the community upon the development of personality and more particularly upon the development of psychopathological character traits. They have argued that the criteria of mental abnormality are not definitely fixed categories but are determined by the general customs of the community. Those very characteristics and modes of behavior which in one culture are regarded as psychopathic, are found in other cultures to be diagnostic of normality and even of social leadership. An interesting speculation arising from this anthropological data is whether psychopathic behavior is primarily a function of the individual or of the culture of which he is a member.

Our investigations indicate that true psychopathic behavior is not to any marked extent culturally determined. Changing the social environment will not transform a psychotic individual into a normal individual or vice versa. An individual is psychotic not because his behavior is incompatible with his culture but because he cannot regulate his behavior to conform with social demands. The crucial factor is the lack of adaptability in the psychopath. The homicidal patient transferred to a homicidal culture would in all probability have a brief career, since his killing would be done in a fashion which would be considered irrational by that community.

Furthermore the resemblance between psychotic behavior and culturally determined "psychopathology" is very superficial. For example an Indian living in a "paranoid culture" does not really behave in a psychotic manner. He neither flees from his village to escape from his persecutors nor does he make homicidal attempts. For him the paranoid performance is a ritual following a fixed cultural pattern. The inability of the individual to follow this ritual constitutes the diagnosis of abnormality.

ATTITUDES

CHAIRMAN, FLOYD H. ALLPORT, SYRACUSE UNIVERSITY

Atmosphere and Contrast Effect: Directional Tendencies in Judgment and Reasoning. L. LONG and S. B. SELLS, Columbia University.

The purpose of this paper is to clarify the relationship between atmosphere effect and contrast effect and to describe generally the function of directional tendencies in judgment and reasoning. Atmosphere effect and contrast effect are both descriptions of behavior involving mental sets arising spontaneously within given situations in which specific directions are imposed upon judgments. Atmosphere effect has been defined as "a temporary set of the individual, arising within a situation (problem) to complete a task with that one of several alternative responses (an inference or judgment) which is most similar to the general trend or tone of the whole situation (problem)." "Atmosphere effect" has recently been demonstrated in syllogistic reasoning, where subjects have chosen the false conclusion to a set of premises which was most similar in quality to the premises. In another recent research on judgment of auditory intensities, "contrast effect" was demonstrated when it was found that under certain conditions stimuli opposed each other in such a way that a weak (low) stimulus preceded by a strong (loud) stimulus was judged weaker than its objective equivalent, and vice versa. Superficially it would appear that these 2 directional effects involve opposing principles. However, careful consideration of the data, and of the processes involved in both cases, indicates that the basic processes are quite similar in atmosphere effect and contrast effect, and that these processes are probably fundamental to all sets involved in judgment, each being manifested according to the conditions imposed by each specific behavior situation.

Gullibility in Relation to Intelligence, Experience, and Training. S. B. SELLS, Columbia University.

In a recent publication the author has presented evidence from experiments on reasoning with syllogistic problems in support of the hypothesis that there is a tendency-to-agree (called "gullibility") which is negatively associated with intelligence.

This evidence is based upon the ratio of errors of selected bright and dull groups in making true-false judgments of both *valid* and *invalid* syllogisms. The tendency-to-agree hypothesis is supported

by the fact that the ratio is higher in the case of invalid problems, where agreement leads to error, than in that of valid problems, where agreement leads to success. The ratio of errors in bright and dull groups on invalid problems was 1 to 3, while on valid problems it was 1 to 1.7.

The purpose of the present paper is to extend this hypothesis by suggesting that gullibility is a function, not only of intelligence, but of training and experience as well. The intelligence test, such as CAVD, used in the above mentioned study, measures individuals' ability with respect to a sphere of reference defined by the psychological function which the test purports to measure. Abilities may be and are measured with respect to other spheres of reference, and individuals' abilities vary from one sphere to another independently of intelligence. One may be versed in psychology but not in mathematics or physics. The hypothesis is thus restated to the effect that not only is the tendency-to-agree negatively associated with intelligence, but that it is negatively associated with ability (and thus training and experience) in any sphere of reference. The discussion will present some evidence from diversified sources in support of this hypothesis.

Dominance-Feeling, Behavior and Status in Women. A. H. MASLOW, Brooklyn College.

This paper reports one section of a series of researches on the nature of dominance and its relations to the total personality. Seventy-five women were subjected to a series of intensive interviews designed (1) to get a fair picture of the total personality, (2) to understand the rôle that dominance-feeling played in the personality, and (3) to discover relationships between social status and dominance-feeling and dominance behavior.

Dominance-feeling is best envisaged as self-confidence, or self-assurance. Women high in this feeling esteem themselves highly, feel superior, capable, forceful and strong. They lack inferiority feelings, shyness, timidity, and are not easily embarrassed. Low dominance-feeling consists of the absence or opposite of these feelings. Inferiority feelings are usually present and are not necessarily related to matter-of-fact superiority or inferiority.

Dominant or subordinate status in inter-individual relationships is determined by comparative level of dominance-feeling, but also by cultural status. A person in dominance status leads, suggests, orders or inhibits the behavior of the subordinate one, and in general, behaves without deference to the latter.

Dominance behavior is not closely correlated with dominance-feeling since such behavior is a resultant of the influences of dominance-feeling, dominance status (however determined), cultural expectations, and compensatory trends. Criteria are given for differentiating spontaneous dominance behavior from overcompensatory dominance behavior. A few behaviors universally correlated with dominance status are discussed but more stress is laid on cultural differences in such behavior.

Dominance-feeling may be expressed in behavior in at least 3 ways comparable to types described for infra-human primates by Maslow, and for children by Bühler.

Dominance feeling is independent of (1) sexual drive, (2) activity, and (3) sociability. Evidence from factor analysis studies supports this finding.

Factors Related to Feeling of Dominance. PHILIP EISENBERG, Columbia University.

The attempt was made to discover some of the social factors which are related to the feeling of dominance. The Maslow "Social Personality Inventory," designed to measure feeling of dominance, was administered to a group of 216 Columbia men, and 235 Barnard women, and, in addition, personal data were obtained.

By the use of correlation techniques, comparisons of averages, and comparisons of extreme groups, it was found that for both groups the following factors were *not* related to feeling of dominance: height, weight, height-weight ratio, color of the eyes, color of the hair, class in college, and age.

For the men's group it was found that position in the family was related. First children and only children tended to be more dominant than children holding other positions. Youngest children tended to be less dominant. These results were not as clear for the women's group.

Using Beckman's Occupational Scale, a tendency was revealed for women whose fathers are of a higher occupational level to be more dominant. This was not clear for the men's group. However, a more definite relationship would probably have been indicated if our groups were more heterogeneous.

For both men and women, it was found that those who went to private school were more dominant than those who went to public school. Jewish students, both men and women, were more dominant than the Protestant and Catholic groups.

By analysis of extreme groups, it was found that the highly dominant individuals tended to make more voluntary comments on the test than the non-dominant. There was an even greater difference in the type of comment that was made.

Experimental Studies of Prestige Suggestion. HADLEY CANTRIL, Princeton University.

The influence and nature of prestige suggestion were studied in 4 separate investigations, similar in method but varying in content. In each investigation a comparison was made between subjects' attitudes toward the names of people prominent in a particular field, and statements, poems, or musical selections attributed to those persons. The bearing of the results on the theory and nature of suggestion will be discussed.

The Organization of Attitudes. S. E. ASCH, Brooklyn College; HELEN BLOCK, Brooklyn College, and MAX HERTZMAN, College of the City of New York.

The purpose of these studies was to analyze the dynamic principles influencing the organization of attitudes. Our first problem was concerned with the degree to which each individual's attitudes were organized under conditions which required the subject to evolve his own standards of judgment. A variety of different experiments was performed with over 300 subjects, employing 4 types of materials at varying levels of complexity: photographs, list of professions, names of political figures, and slogans. Essentially the same procedure was used in all experiments: A group of subjects was asked to rank 10 items of a category (*e.g.* 10 professions) in the order in which they possessed certain characteristics (*e.g.* social usefulness, idealism, intelligence, etc.). The intercorrelations between the rankings were positive and fairly high for the great majority of subjects, indicating that the standards evolved by each individual for any one item of a category were highly interrelated. These results—known in the literature as the "halo" effect—were obtained in all experiments. The subjects also showed high agreement among themselves, demonstrating the existence of group standards.

In the second series of experiments, the subjects were provided with arbitrary rankings of one trait (differing in specified ways from the group standards) and then required to rank the items for the remaining traits. Our problem was to study the influence of the new scale on the resulting judgments. The results show that the externally introduced standard: (1) changed the absolute rankings

of the remaining traits; (2) changed the interrelations between the judgments. Further, the degree of change was a function of the difference between the arbitrary scale and the previously determined group standards. Thus, a zero correlation between the 2 standards produced a disruption in the organization of the judgments which was even more pronounced when the correlation between the standards was negative. It is concluded that a single judgment of an individual can be understood only as a function of an organized set of standards.

Learning and Memory

Chairman, Clark L. Hull, Yale University

Learning Which Modifies an Animal's Subsequent Capacity for Learning. Leonard Carmichael, University of Rochester.

This experiment reports the setting up of a simple maze habit which renders the subsequent establishment of a different but exactly comparable habit difficult if not impossible. The maze pattern used has been previously described by the writer in an experiment with cats. In the present experiment 10 rats learned that at the end of a common segment a left turn was required if the entrance turn had been left, a right turn if the entrance turn had been right. After this habit had been established to regular alternations, the problem was changed, using the same maze but requiring the animal to turn left at the end of the common element if the entrance turn had been right and right if the entrance turn had been left. Description of the so-called "neurotic behavior" shown by animals at the choice point in the second maze is given. The significance of the time scores of the 2 mazes is also discussed. Results are presented as curves showing average performance by trials and also as modified Vincent curves. At the conclusion of 156 runs on the second maze, the animals had shown no sign of mastering it and had not reached a level of performance equal to that reached at the end of 48 runs on the original maze. The performance of the animals on the second maze can only be attributed to the persistence of learned reactions acquired on the first maze. An explanation of these results on the basis of a mechanistic theory of learning is suggested and the difficulty of explaining them in terms of "hypotheses" or "insight" or in a "topological" system is indicated.

The author wishes to express his gratitude to Mr. H. A. Israel for assistance in this experiment.

Forced Movement or Insight. ZING YANG KUO, University of Rochester.

The experiments reported in this paper are intended to demonstrate that what has been regarded as manifestation of "insight" in the rat is a result of faulty laboratory technique and that the so-called "insightful" behavior, when adequately analyzed, belongs in the category of forced movement. The experiments by Hsiao and by Tolman and Honzik, demonstrating the presence of insight in the rat, were repeated with different methods of procedure. The results clearly show that the rat actually has no insight. What Hsiao and Tolman and Honzik have reported as indication of insight is merely a result of the functioning of the factor of frequency, which was overlooked by these investigators. Frequency in this type of experiment works in 2 ways. On the one hand, it tends to make the rat form a definite position habit of turning to the right or to the left. On the other, when the rats have normal vision and the pathways are broad, frequency tends to make the rat in the "insight test" choose the path which it was forced to go through most frequently during the preliminary trials, that is, before the "insight test." In these experiments the experimenter was able to manipulate according to his wishes the presence or absence of the so-called "insight" by merely controlling the factor of frequency. In the maze of the type similar to those used by Hsiao and by Tolman and Honzik, whether walled or elevated, the rat, whether with normal vision or with eyes sewn and plastered, may behave "insightfully" or "uninsightfully." This depends upon the frequency with which the rat is forced to enter the different paths in preliminary training. We call this type of behavior "forced movement" instead of insight, or trial and error because it is what it has been forced to be.

"Reminiscence" and Distribution of Practice in Serial and Paired-Associate Learning. CARL IVER HOVLAND, Yale University.

If the pronounced initial improvement in retention which follows serial learning of nonsense syllables by massed practice (Ward, Hovland) results from the dissipation of "inhibition" produced by overlapping excitatory tendencies between the syllables throughout the series (Hull), this effect ("reminiscence") should be markedly reduced in paired-associate learning. With paired associates, connections are established between the members of the pairs, but the experimental procedure precludes, to a considerable extent, remote associations.

Comparison was made of reminiscence following learning by these discrete methods. Forty college students learned serial and paired-associate lists on alternate days in counterbalanced practice order. The 2 types of lists were equated, in a preliminary experiment, in terms of the trials for perfect recitation. The effect of the rest pause, during which the subjects named colors, was determined at 3 stages of learning. When the lists were learned by the serial method both recall and relearning scores were better when testing followed the rest pause than when testing occurred immediately after learning. Improvement was slight, but statistically significant, in serial learning when testing either followed one presentation of the list or followed perfect recitation. At neither of these stages was reminiscence obtained in paired-associate learning. Following an intermediate degree of learning, in which the improvement in relearning scores averaged over 20% for serial lists, the reminiscence with paired associates was only 3%. In learning where no reminiscence is obtainable, distributed practice should be no more efficient than massed. This deduction was verified for paired-associate learning. Twenty-four subjects learned serial and paired-associate lists by both massed and distributed practice. Distributed practice was 18% more efficient in serial learning; no advantage of distribution of practice was obtained with paired-associate series.

Associative Latency as a Function of Interpolated Learning. GRACE O. MCGEOCH and JOHN A. MCGEOCH, Wesleyan University.

This experiment was designed to test the question as to whether or not the latency of the responses when relearning the original material is a function of the character of the interpolated learning material. McGeoch and McDonald have shown that the degree of similarity between the original and the interpolated materials is a significant factor in the rate of relearning the original material.

The conditions of this experiment were repeated with 20 subjects, Wesleyan college students.

The latencies obtained under the rest condition will be compared with those obtained under the work conditions and a description of the graphic recorder, the apparatus which measured the latency of responses, will be given.

Maze Behavior and Maze Design. H. A. WITKIN, New York University.

This study was designed to examine more precisely the functional relationship between initial choice-point behavior and the spatial dis-

tribution of immediately preceding alleys as it has been demonstrated in recent maze experiments. Three simple maze designs were employed. In one situation (maze *A*) the 2 elbows preceding the choice-point turned in the same direction (making a U-succession of alleys); in maze *B* the elbows turned in opposite directions; in maze *C* the choice-point was preceded by a single elbow (resulting in an L-sequence of alleys). From the choice-point in any of the 3 situations the animal could reach a food-box by taking either of 2 alternative pathways. In all 3 situations the elbow just preceding the choice-point involved a *left* turn. Approximately 60 pigmented rats were used in each of the 3 maze situations.

Following a period of preliminary training upon a simple T-maze each animal was submitted to an *initial test* (12 trials given on 2 successive days of running) in one of the maze situations. Animals run on the *A*-type maze were then given 12 trials on the *C*-type; animals run on the *B*- or *C*-type were given 12 trials on the *A*-type. The latter training period constituted the *transfer tests*.

During *initial tests* on each of the 3 designs the *right-going alternative* at the choice-point was favored by the respective groups as follows: maze *A*, in 86.0% of total encounters; maze *B*, in 58.8%; maze *C*, in 63.9%. (Further, while a highly uniform type of choice-point behavior was observed among animals run on the *A* situation, animals trained on the *B* and *C* arrangements exhibited a very wide range in behavior.) Upon *transfer* to the *C*-type maze animals previously run on the *A* maze favored the right alternative in 68.9% of total encounters (a drop of 17.1%); animals transferred from the *B* maze to the *A* maze chose the same alternative in 77.5% of the encounters (a rise of 18.7%); while animals transferred from the *C* maze to the *A* maze chose the right-going pathway in 86.3% of the total encounters (a rise of 22.4%).

These results indicate that initial choice-point behavior is a function of the spatial distribution of at least the 3 preceding alleys. Since the forward-going-tendency hypothesis fails to recognize the importance for initial choice-point behavior of alley features other than the immediately preceding elbow, it is rejected in favor of the more analytical and comprehensive centrifugal-swing theory. The results emphasize the fact that a knowledge of the rôle played by various alley features in influencing initial choice-point behavior is of fundamental importance in evaluating the results of maze studies and in the planning of useful maze situations.

Direction in Recognition. N. J. HANAWALT, New Jersey College for Women.

This study aims to test the validity of Koffka's principle of 'consistent direction' in the memory trace by the method of recognition. Two measures of original direction are taken: (1) from perception, and (2) from memory. Wulf's original figures are used. On the recognition blank, the stimulus figure appears along with 9 similar figures, most of which deviate from the original by different degrees of 'sharpening' and 'leveling.' The recognition sheets were constructed from smoothed reproductions.

1. *Original Measure from Perception.* While the stimulus figure was on the screen, 90 subjects selected the figure in each of the 24 recognition groups which looked most like the figure on the screen. On the average, each subject selected the wrong figure in 6 out of the 24 groups. Since these errors were in one direction or the other, they afforded a measure of the later direction of the original perception. Twelve weeks later the same subjects marked the recognition blanks again, this time from memory. A comparison of these 2 markings made possible a check on consistency of direction.

2. *Original Measure from Memory.* In this group, the subjects marked the recognition blanks 15 minutes after the presentation of the stimulus figures. The errors in this marking were taken as measures of original direction. After 8 weeks, they marked the recognition blanks again. As above, this second marking served as a check on direction.

Conclusions. Whether the measure of original direction is taken from perception (the figure present while the subjects are marking the recognition blank) or from memory, the results fail to substantiate Koffka's principle of "consistent direction."

The Use of Tachistoscopic Exercises in the Improvement of Reading Speed. C. O. WEBER, Wells College, Aurora, New York.

Initial and final measurements of reading comprehension and reading speed (The Iowa Advanced Silent Reading Test, Forms A and B, together with a test devised for the purpose of the experiment) were given to 22 experimental and 20 control subjects of college grade. Between the initial and final tests, the experimental subjects were given periodic practice in identifying forms, in reading sense and nonsense materials, and in responding to true-false statements. The practice materials were exposed for critical time intervals.

In the final tests the experimental subjects made a residual gain over the controls of 4% in reading comprehension, and a residual gain of 24% in reading speed. Twelve days intervened between initial and final tests. The experiment also gives results bearing on such questions as: (1) the persistence of such improvements with passing time, (2) the best materials and procedures for practice work, and (3) the relations between improvability and general intelligence and initial reading speed.

Brain Processes

Chairman, Carlyle Jacobsen, Yale University

Bilateral Brain Potential Records and Lateral Dominance in Identical Twins. Edward T. Raney, Brown University.

Comparative data on lateral dominance is presented on 17 sets of identical twins and 43 control children of age range from 7 to 16. On the Phi-test of lateral asymmetry in binocular perception, one twin was right and the other left dominant in all cases tested. In the control group, 88% were right and 12% were left dominant. Distribution of right and left dominance in manual habit preferences and in eyedness is quite similar for the twins and for the controls. The twins showed a greater percentage of mixed dominance on the Van Riper critical angle board test.

On the whole, the group of twins who were right dominant according to the Phi test showed a greater percentage of alpha wave activity in the encephalograms from the right precentral region than did the left dominant group, and vice versa. No such relationships were found with the percentages of alpha wave activity from the occipital region, or with alpha wave frequencies. Comparing the twins by pairs, it was found that the similarity in percentage of alpha wave activity is no greater than chance. There is, however, a definite positive resemblance between twins in terms of the alpha wave frequencies from the same side of the head.

It is concluded that, when the proper dominance tests which minimize learned reactions are used, our data on identical twins demonstrate a genetic basis of a fundamental organic determination of certain aspects of lateral dominance.

The Origin and Genesis of Rhythm in the Electroencephalogram. J. Roy Smith, Babies Hospital, New York City.

Employing standard technique, data have been obtained from 57 normal children ranging in age from a few days to 17 years, including

13 infants and young children upon whom repeated experiments have been carried out at short intervals over a period of several months as part of a prospective longitudinal study.

Records are taken with the child lying at rest and in semi-darkness within a shielded chamber. With both electrodes placed over the occipital region, potentials recorded during the first 2 months are predominantly random in appearance. At about the tenth week short sequences of periodic waves varying between 3 and 4 oscillations per second begin to appear. By the end of the fourth month these rhythmic waves are usually well established and thereafter they increase gradually in frequency, attaining the adult level at about 8 years. These data are found to fit a well-known growth equation and certain facts arising from this fit appear to be of some theoretical significance.

Simultaneous tracings obtained with electrodes placed over other underlying cortical areas show periodic waves long before the third month. Repeated experiments show that these waves may be present at birth or within a few days after birth, that they may be of various frequencies and amplitudes and that they are most probably localized in the motor region since the optimal condition for their appearance is that both electrodes should be placed over this area. Two main frequency groups have been observed: (1) a slow group ranging from 4 to 8 per second and approximating alpha amplitude; and (2) a faster group including smaller waves ranging from 12 to 15 per second.

The existence of well-defined rhythmic activity at or soon after birth suggests the probable intra-uterine origin of this activity and the possibility of some degree of correlative cortical function.

Brain Potentials in the Hereditary Type of Mental Deficiency. GEORGE KREEZER and FRANKLIN W. SMITH, The Training School at Vineland.

The present paper provides a preliminary report on the characteristics of the electroencephalogram in the hereditary type of mental deficiency. The main questions investigated in this experimental group were: (1) What relation is there, if any, between the properties of the electroencephalogram and Binet mental age? (2) Are there special properties of the electroencephalogram characteristic of the hereditary type of mental deficiency?

The experimental group consisted of 58 subjects classified as showing mental deficiency which was probably hereditary in origin. These subjects ranged in mental age from 3 to 10 years. Records of

subjects above and below 17 years of age were analyzed separately in order to control the factor of chronological age. Records of 41 Mongolian mental defectives and 25 mentally normal subjects were also available for determining the influence of certain other variables.

Records of electric potentials were obtained with active electrode over each of 3 brain areas: occipital, motor, and frontal. Recording was done photographically, by means of a Westinghouse oscillograph, and also by means of an ink-writing oscillograph (undulator). The chief properties of the records examined were percentage of time occupied by regular sequences of alpha waves, and frequency and amplitude of waves in regular sequences.

Some tentative conclusions are: (1) Marked individual differences occur among subjects of almost all mental age levels. These differences increase the difficulty of determining the influence of other variables. (2) There is a decrease in prevalence of regular alpha sequences at the lower mental ages. (3) There are indications that differences in the character of the electroencephalogram are associated with differences in type of mental deficiency.

Vasomotor Responses and Emotional Overreaction in Patients with Thalamic Syndrome. DONALD G. MARQUIS, Yale University.

The theory that in emotion the thalamus is released from cortical control is based on 2 fundamental observations: (1) that decorticate animals respond to painful or emotional stimuli by an exaggerated reaction involving a generalized sympathetic discharge; and (2) that in certain clinical cases exhibiting emotional overreaction to painful stimuli (thalamic syndrome), it has been found that the pathological lesion sometimes involved the lateral portion of the thalamus, and presumably interrupted a cortico-thalamic inhibitory pathway. The first fact deals only with observed behavioral response; the second, only with the reported affective consciousness. If the 2 facts have any mutual relation it should be possible to demonstrate an increased sympathetic reaction in patients with the hyperpathia of the thalamic syndrome.

For this purpose a series of patients was studied at the National Hospital, Queen Square, London, in collaboration with Dr. D. Williams and Dr. E. A. Carmichael. The vasoconstriction response to measured painful and thermal stimuli was recorded photographically from finger plethysmographs. In patients with a characteristic thalamic syndrome it was found that there was no difference between the vasomotor responses to stimuli on the normal side and on the affected side of the body. That the methods used in the investigation

will reveal a difference if it is present is demonstrated by the exaggerated vasomotor response in patients whose hyperpathia is due to a lesion in the ascending sensory pathway, *e.g.* spinal cord, dorsal roots, or peripheral nerves. These results indicate that the emotional overreaction of decorticate animals and of patients with thalamic syndrome is not the same phenomenon in the 2 cases, and suggest that modification of the concept of thalamic release is necessary.

The Concepts of Equipotential Brain Functioning and Specific Localization as Applied to Human Neurosurgery. RUSSELL MEYERS, New York University. (Introduced by P. D. Stout.)

A review of the literature reveals that investigators of cerebral activity have favored now a doctrine in which "higher functions" are assigned to circumscribed brain areas and again a doctrine in which these functions are held to be subtended by a dynamic equipotential "mass action" of the brain.

The present-day neurologist, neurosurgeon and psychiatrist, appreciating that advance in the homely problems of diagnosis and treatment of aberrations of nervous and psychologic functions depends upon progress made by their academic colleagues, seek to avail themselves of the data arrived at by the latter. Their questions are two: Does the clinical exercise of the interpretations offered by the pure scientist enable us to state reliably whether a cerebral lesion is present? If so, precisely where is it located? The present study, based on a correlation of the clinical, operative, and autopsy findings in 10 representative cases of brain tumor, indicates that adherence to either the "equipotentiality" or "specific localization" views may prove insufficient, if not actually misleading. The writer accounts for the disparities between the site of brain lesion as determined by the clinical application of academic considerations and its actual site as revealed at operation or autopsy by identifying 13 pathophysiological factors. It is concluded that the present insufficiency of knowledge demands that aerography, electroencephalography and angiography be employed to supplement academic and clinical data.

Visually Controlled Responses Under Conditions of Stimulation Associated with Apparent Movement in the Cat. KARL U. SMITH, University of Rochester.

In the present investigation, the visual capacity of the cat in responding to apparent movement was studied in relation to forced eye-movements brought about by rotation of the visual field. The conditions of apparent movement were obtained by means of a large

rotating drum, lined with black and white striations, which were intermittently illuminated by means of a stroboscope. Slow apparent movement of the visual field, either clockwise or counter-clockwise, was secured under circumstances in which the actual movement of the stripes and the rate of intermittent stimulation were less than the flicker fusion frequency of the cat's eye at the level of illumination employed. The animal was restrained in a box-holder and its head located at a point near the center of the drum. The eye-movements induced by the apparent movement of the stripes were recorded electrically.

The results of the experiment show that circumstances of apparent movement of the visual field are as effective in producing visually controlled nystagmus in the cat as are the conditions of real movement. Eye-movement records, showing the presence of such reactions as well as the after nystagmus produced by the apparent movement are described. These records are compared to similar ones obtained under conditions of real-movement stimulation. Preliminary observations on the effects of complete extirpation of the striate cortex upon such apparent movement vision in the cat are discussed.

The Effects of Bilateral Removal of the Striate Areas of the Cortex upon Visually Controlled Nystagmus in the Cat. SAMUEL BOJAR, University of Rochester. (Introduced by C. S. Bridgman.)

The occurrence of visually controlled nystagmus at varying speeds of rotation of the visual field was determined in 3 cats after extirpation of the striate areas of the cortex. The apparatus used in this experiment consisted of a large drum, in the form of a truncated sphere, the walls of which presented alternate black and white lines which subtended equal visual angles when the animal's head was located near the center of rotation. The drum was of such a size that the moving stripes completely filled the visual field. The eye-movements of the animals were recorded electrically both during and after rotation of the drum.

The visual capacity of the animal in this situation may be considered in terms of its ability to respond to a given number of stripes passing a given point in the visual field per second. Two animals gave definite evidence of eye-movement with speeds of rotation varying between 4 and 11 stripes per second (angular velocity 28.8° and 79.2° per second). The third animal responded to speeds of

rotation as great as 15 stripes per second (angular velocity 108° per second). The presence of definite after-nystagmus was observed in 2 animals at the slower speeds of rotation.

These results are compared to similar data obtained on 8 normal animals, with special reference to the presence of after-nystagmus, and the velocity and frequency of the eye-movements at the different speeds of rotation.

Comparative Psychology

Chairman, Walter R. Miles, Yale University

Factors Influencing the Rate of Eating of the White Rat. P. M. Fitts, University of Rochester.

In the course of experiments on the physiological basis and effect upon behavior of various degrees of hunger, it was necessary as a preliminary step to determine quantitatively the influence of various factors upon the rate of eating. Quantitative records have been secured from 36 animals over a period of 2 months in order to determine the effect upon the rate of eating of: (1) 6, 12, 24, and 48 hours of food deprivation; (2) previous conditioning to a 24-hour feeding schedule as contrasted with a feeding rhythm determined spontaneously by the animal; (3) reduction of general body weight; (4) solid as contrasted with granular food. In addition to the rate of eating several quantitative measures were also made of the variability of behavior.

All critical trials were for 1 hour, during which time the animals were in semi-soundproof, lightproof, ventilated experimental boxes. Six hundredths of a gram of either solid or granular food was secured by the animal each time it pressed a small lever in the box. An electro-mechanical recording device gave quantitative time-response record curves of the eating behavior.

Quantitative results are presented. These results indicate that the specific history of the animal for some time before the critical tests has an influence upon the rate of eating as well as does the immediately preceding period of deprivation. Variability is a function of deprivation and varies inversely with the length of deprivation. The period of deprivation which will lead to the maximal rate of eating is related to general body weight, the conditioned feeding rhythm, and age. Change of rate as the condition of satiation is neared is to some extent a function of the type of food eaten.

An Investigation of the Rate and Amount of Thirst Motivated Activity in the Cat. MARGARET FOLTZ, University of Rochester. (Introduced by K. U. Smith.)

Number-*versus*-time graphs of a learned response and its extinction in the cat were obtained after 24- and 48-hour periods of water deprivation. The animal obtained a constant amount of liquid upon depression of a small lever located within a sound-resisting box. The only liquid included in the diet of the 6 subjects used was the milk procured in the experimental station.

The typical drinking curve is a straight line, or a broken curve of several straight lines, interrupted by periods of no activity. Among several hundred curves obtained, a very small number exhibit a gradual deceleration of activity, rather than the typical abrupt stop. The longer period of deprivation served to increase the total number of responses but did not produce any marked increase in rate.

Extinction curves were obtained after each of the 2 periods of water deprivation. No differences were noted in the rates of extinction corresponding to the 24- and 48-hour deprivation periods. In general, a greater number of responses occurred as a result of the longer deprivation. The most significant aspects of these curves are the rapidity of extinction and the small number of responses made before cessation of activity.

In regard to the data obtained upon the drinking behavior of the cat, the abrupt termination of the activity at the point of satiation, without any prior deceleration in the rate of response, indicates that the thirst-motivated activity of the cat is a function of a relatively specific set of stimulus factors rather than of a gradually changing physiological state. The curves of extinction obtained here on the cat, when compared to similar data on other animals, seem to point to important phylogenetic differences in rate and amount of response during periods of experimental extinction.

The Use of the "Comfort" Drive as Motivation in Visual Discrimination by the Infant Chimpanzee. T. L. MCCULLOCH, Yale University.

A notable characteristic of the chimpanzee infant is its dependence upon the near presence of, or actual physical contact with, its mother, caretaker, or cage associate. The establishment of a peculiarly strong affective attachment for paper toweling by one infant suggested that the " comfort " drive might be used to advantage as a source of motivation in a number of experimental situations. Such applicability

would have considerable practical value, inasmuch as the most commonly used source of motivation, the hunger drive, is often unsatisfactory for the infant chimpanzee. Results of experiments indicate that the " comfort " drive is effective as motivation in certain problems with infant chimpanzees. So far, this drive has been used with delayed reaction and with the establishment of visual discrimination habits.

In the investigation here reported 2 infants were trained upon a black-white discrimination habit, and subsequent reversals thereof, with an object of the " comfort " drive as reënforcing agent. Variations of motivating conditions were introduced, and the effects of such variations analyzed. Limitations for experimental utilization of the " comfort " drive are noted.

Reinforcement of " Reward-Expectancy " in Delayed Response of Young Chimpanzees. F. M. FLETCHER and H. W. NISSEN, Yale University.

Previous work from this laboratory has shown (a) that the rewards given in delayed response trials have a general or perseverative effect on subsequent responses, and (b) that the specific nature of the incentive (large or small) used on a given trial influences the accuracy of the response of that trial (which precedes ingestion of that incentive-reward). The present experiment was designed to investigate one of the primary conditions of the latter phenomenon, which may be termed " reward-expectancy."

Three young chimpanzees worked for food incentives in a multiple-box delayed response apparatus. Large and small incentives (1/4 and 1/32 orange) were presented equally often but in mixed order throughout the experiment. Reward-expectancy was reinforced and not reinforced in alternate series of trials. During the reinforced series the subject always received as reward for correct response the piece of orange (large or small) which it previously had seen placed into the container. In the unreinforced series the subject always received a small reward for correct response, regardless of the size of the incentive which previously had been presented. Each series consisted of from 320 to 960 trials, a series being continued until the difference between the performance levels (number of errors) for large and small incentives remained reasonably constant for 320 trials. The general level of performance was maintained favorable for differentiation by varying the difficulty of the problem in terms of the number of boxes used and the length of the delay

interposed. Emotional manifestations of reward-expectancy were also recorded by a rating scale technique.

The results indicate that the expectancy value of the incentive is a function of the reinforcement given to such expectancy, through actual reward, in previous trials.

Food-Tokens as Incentives for Learning by Chimpanzees. JOHN T. COWLES, Yale University.

Young chimpanzees were trained to associate previously indifferent objects with food reward, using a work device from which they obtained discs exchangeable for raisins in an automatic vender. They were then required to work for groups of these tokens before being allowed to exchange them for food. After this preliminary training, the animals were taken to a different room and were given sessions of trials on standard learning problems: simple and complex position habits, visual discriminations, and delayed response. Only tokens were received for correct responses in the learning problems; these were carried by the animal to the exchange room after a group of 10 to 30 trials, and were there exchanged for food.

The data indicate that these tokens, with delayed exchange for food, will operate as selective reinforcing agents in the learning of a variety of habits requiring repeated trials before mastery. Day to day motivation remains consistently high. It is evident that this technique easily lends itself to the study of indirect rewards not only in the *performance* of established habits or work responses, as has been shown by Wolfe, but also in the *acquisition* of new habits.

This experiment is the first part of a larger investigation which is directed at an evaluation of the specific reinforcing strength of the food-token by comparing it with direct food reward and with discs which deliver no food upon exchange. Various conditions of exchange are being studied.

An Experimental Study of the Ontogeny of Vision in the Rabbit. JOHN WARKENTIN, University of Rochester.

This investigation reports the development of different visually determined responses in new-born albino and pigmented rabbits, and some of the factors contributing to the regulation of this development.

Visual acuity measures were secured by means of the rotating-visual-field method previously described in work with kittens. Daily tests with 14 rabbits indicated that oculocephalogyric responses could first be elicited in response to alternate white and black stripes subtending a visual angle of 12 degrees in the experimental cylinder at

the age of 10 to 16 days. The highest degree of visual acuity measured corresponded to a visual angle of 11 minutes, and was attained by the pigmented rabbits at the age of about 16 days. The albino rabbits never exceeded a visual acuity of 43 minutes of arc, which was demonstrated at the age of 16 to 24 days. Increase in visual acuity in young rabbits seems to occur in saltatory steps, rather than gradually as in kittens.

Data will be presented as to the ages when the eyes opened, when the corneal reflex appeared, and when the lid and pupil reflexes were elicited in response to light stimulation.

The eyelids of some members of each litter were opened surgically at ages of 2 and 3 days. Such premature opening had no marked effect on the age at which visually determined oculocephalogyric responses could first be elicited.

The problem whether the clearing of the optic media is related to the development of visual acuity was investigated by daily observations with an ophthalmoscope. It was found that the optic media begin to become transparent at about the same period when the first oculocephalogyric responses were observed, but it is probable that this change is not the only factor to be considered as determining the maturation of the visual mechanism.

Film Showing the Ontogeny of Embryonic Behavior in the Chick. Zing Yang Kuo and L. Carmichael, University of Rochester.

This 200-foot film shows the behavior of the chick embryo from the time when the heart beat alone is visible through the time at which amnion contractions, head movements, and limb movements are at their maximum. Activity of the type immediately preceding hatching is also shown. Photographs are for the most part taken through the inner shell membrane which is rendered transparent by the method previously described by Kuo.

Psychometrics

Chairman, Percival M. Symonds, Columbia University

The Natural Number of Categories in Absolute Judgment. John Volkmann, Harvard University.

In most psychophysical experiments the number of categories which the observer is to use is specified in the instructions. Under some conditions, however, this number may be reported as being too small or too large, so that apparently there is an optimal or natural

number of categories which is a function of the conditions of judgment. The task of this experiment was to determine the natural number of categories as a function of stimulus-range, which is defined as the difference between the highest stimulus and the lowest one. The use of absolute judgments permits wide variation in the number of categories.

Three observers made absolute judgments upon the inclination of lines of light exposed in a dark box. The experimenter controlled the inclination of the stimulus lines. The stimulus range varied in several steps from 4° to 80°. After watching several presentations of the stimuli, the observer selected a preliminary number of categories, and gave judgments in this number. To illustrate, if he selected 3 categories, his judgment might be *one, two,* or *three.* He then tried using more categories or fewer until he declared himself satisfied.

The optimal or natural number of categories increases with the stimulus range in accordance with a function of sigmoid shape. There is evidence of an additional independent variable, whose action tends to produce a whole family of sigmoid curves. A possible explanation, as yet unverified, is that the observer varies the number of categories until the average certainty of his judgments reaches some given value; however, this value might change at any time, thus producing different curves of the same general shape.

The Arrangement of Statements in an Attitude Scale. ABRAHAM KROLL and JACK W. DUNLAP, Fordham University.

The statements in attitude scales have been presented in 2 ways, in a random order and in a graded order of statements from favorable to unfavorable. The simplicity of scoring the graded arrangement raised the following questions: does this arrangement affect the mean reaction of a group, the variability of the scores, or the reliability of the scale?

The 2 scales used in this investigation were the Peterson-Thurstone War Attitude Scale, and the Kelley-Remmers Generalized Attitude Scale for any Institution adapted to measure the attitude toward war. The subjects were 232 ninth grade boys drawn from a low socio-economic area in New York City, and were for the most part of foreign born parents.

The scales were rearranged, with the consent of the publishers, as to order of arrangement of statements, so that it was possible to administer both scales in serial and in random orders.

There is definite evidence from this investigation that arranging the statements in an attitude scale in serial order as to scale value has no significant effect upon the mean, the standard deviation, or the reliability coefficient. Since the scoring is so materially affected by the serial arrangement, the statements in an attitude scale should be arranged in serial order of scale values.

A New Scoring System for the Minnesota Clerical Test. BEATRICE CANDEE and MILTON BLUM, Vocational Service for Juniors, New York.

The scoring system in use at the present time does not weight accuracy to the extent that job specifications of clerical work demand. Our experience in hiring clerks has shown that when concessions must be made, they are made on speed but not on accuracy. The scoring technique of right minus wrong does not apply to the test because an answer is not determined by chance, but is a result of a comparison of 2 series of items. The answer is based upon an individual's ability to pay attention to all the parts of the numbers or names to be compared.

The scoring system proposed considers speed and accuracy as separate factors and tends to eliminate the sources of error mentioned. The test was administered to 1,190 boys and 770 girls. Percentiles were developed for the number of items attempted. A letter grade of A, B, C, D, or E, was assigned to each result depending solely upon the number of errors committed. Thus each scoring represents both speed and accuracy of the person taking the test. Under the old system 2 clients could obtain a score of 120; one with no errors, and the other with 10 errors. Under the new system the first client would obtain a score that would differentiate him clearly from the second client.

This scoring system must be subjected to a test against clerical workers. The percentiles presented here are based upon a general population and are submitted for inspection and use.

Gen-Like: Halo or Reality. IRVING LORGE, Teachers College, Columbia University.

In general, tests or scales of personality, interests, attitudes, and the like require the respondent to indicate his degree of liking or approval for items on a three- to five-point scale. Examples are the *Yes No?* of Bernreuter, the *L I D* of Strong, the *check cross question* of Thurstone, and the *1 2 5 8 9* of Thorndike.

For a group of adults who had been given all these tests and many others, a positive correlation was found between the number of *Yes's* on the Bernreuter and the number of *checks* on the Thurstone, and the number of *L's* on the Strong and the number of *1's* and *2's* on the Thorndike. Similar positive relationships were discovered for the *No, D, cross* and *8+9's* of the various tests. And again positive relationships were found for the *?'s* of Bernreuter, *?'s* of Thurstone and *I's* of Strong and the *5's* of Thorndike.

It is inferred that (1) the method of rating items introduces a special halo effect, and (2) that the tendency to respond by *Yes's, No's* and *?'s* or similar rubrics may be symptomatic of a special aspect of personality.

The author wishes to express his appreciation to Mr. Ray H. Simpson for assistance with this study.

The Fallacy of Measuring Personality by the Same Methods as Intelligence. ZYGMUNT PIOTROWSKI, Columbia University.

The so-called objective measurements of personality are unable to combine objectivity (Warren's dictionary) with validity. The chief reason for this failure seems to be the fact that those measurements are based on essentially the same methodology as measurements of intelligence level. There are fundamental differences between intelligence and personality which require fundamentally different methods of study.

1. All individuals undergo a fairly standardized training of intelligence. This is not so with personality.

2. Intelligence level remains constant and changes are exceptional. Change and development of personality are natural.

3. Intelligence measurement can be verified by special criteria of school and occupational success. Personality measurement cannot be validated by any special type of achievement.

4. The deviation from the norm ("diagnosis") in intelligence measurement can be reliably established without any qualitative description of intelligence. Personality description necessarily precedes any reliable personality diagnosis.

5. It is the degree and type of integration which is important in personality. Similar outward behavior patterns are frequently expressions of very different personality structures. Thus the stimulus-response principle becomes inadequate. If sampling of stimuli is wide, the stimulus-response principle remains sufficient for adequate measurements of intelligence level.

An analysis of the success of various present-day methods leads to the conclusion that the validity of personality description increases with the amount of data given spontaneously by the subject. Improvement therefore should go in the direction of creating methods which would encourage the subjects' spontaneous behavior and which, at the same time, would standardize the environmental condition acting as a background for that behavior.

Remedial Reading in Secondary Schools. ANNETTE BENNETT, New York City Schools.

The need for a program of remedial reading in the secondary schools will be discussed and a summary presented of recent educational surveys which indicate the extent of retardation in reading in the secondary schools of the country.

An analysis will be given of the results of a remedial reading project which was conducted in 15 New York City high schools for the period February–June, 1936. The following points will be developed: (1) Basis for the selection of pupils to be enrolled in the project. (2) Methods of programming the remedial work. (3) An analysis of the intelligence ratings of the retarded readers. (4) Discussion of tests used and gains made in reading skill as a result of tutorial instruction. (5) A comparison of I.Q. ratings of pupils who made satisfactory progress *versus* those who made little or no improvement. (6) Evidence showing the need of a varied attack in a remedial reading program. (7) General summary of the reading difficulties of pupils at the high school level and suggestions for the improvement of a remedial reading program.

Factors in the Repetition and Elimination of Error. J. F. KUBIS, Fordham University.

This is a preliminary report of a larger investigation. Seven tests were administered to 188 girls and 180 boys in grades 4–6 of an elementary school. Papers were scored for errors as well as for correct responses. To determine how and which errors are eliminated with continued application of the same test, 2 of the same tests were administered 3 times, 90 and 30 days after the first application. The errors were tabulated and arranged according to types.

An objective rating scale was constructed for the following characteristics: attentiveness, "nervousness," difficulty in expression, persistence, and impulsiveness. The relations between error, correct scores, and each characteristic were determined.

The results show factors which must be considered in the exact interpretation of error.

Applied Psychology

Chairman, Morris S. Viteles, University of Pennsylvania

Phonetic Symbolism in the Deaf. Grace M. Heider, The Clarke School for the Deaf, Northampton, Massachusetts.

Sapir and Bentley have shown that phonetic combinations have symbolic values. The following experiments were made to determine: (1) whether the deaf can feel these symbolic values in their non-auditory experience of speech; (2) if so, whether the values that they feel parallel those felt by hearing persons whose sensory experience of speech goes beyond that of the deaf.

Two experiments were made. In Experiment I young deaf subjects were taught to pronounce pairs of unfamiliar words. The 2 meanings were then shown by demonstration and the subjects were asked to match words and meanings. In Experiment II the subjects were asked to pronounce pairs of nonsense words contrasting a series of 8 vowels (28 pairs of words) and were asked in each case to tell which of a pair represented the larger, which the smaller of 2 objects. In a second trial they were asked which represented the brighter of 2 objects. This technique had been used by Sapir and Newman with hearing subjects and comparable data were available.

The results of the experiments indicated: (1) that the deaf can feel the symbolic character of their own speech; (2) that their experience is in many cases similar to that of persons who hear; (3) that there are cases in which there is a difference between the symbolic character of phonetic combinations for deaf and for hearing persons. These differences can be directly traced to the lack of the auditory factor in the sensory data of the deaf and to the greater significance of kinesthetic factors.

The Statistics of Card Guessing. Joseph Zubin, College of the City of New York.

Rhine's recent work has reintroduced the ancient problem of the statistics of card guessing. Rhine's statistical procedures have been attacked by several workers but no adequate substitute has yet been offered. This paper analyzes the statistical difficulties of Rhine's method—sampling from a finite universe in which the probability of a hit changes from guess to guess; and the psychological difficulties—the fact that people do not give the perfectly random guesses that would be expected by chance. A statistical technique is worked out

for dealing with a modification of Rhine's procedure in which the subject distributes his guesses in equal proportion among the 5 different cards—in a 5:5:5:5:5 ratio. A simplification of his experiment is also suggested, consisting of the utilization of only 5 cards in each guessing task (instead of the 5 sets of 5 cards which Rhine uses) and the chance hits to be expected are shown to be as follows: the total number of different patterns of guesses is 120. Out of each set of 120 guesses, there are expected by chance 1 pattern with 5 hits, no patterns with 4 hits, 10 patterns with 3 hits, 20 patterns with 2 hits, 45 patterns with 1 hit, and 44 patterns with no hits. The obtained frequency of hits can be compared with the expected, by the chi square technique. The mean number of expected guesses is 1, and the standard deviation is 1. This affords a second but less rigorous way of comparing the obtained with the expected results. Experimental data will be presented in which the new technique is applied.

The Influence of Instructions on the Pattern of Work Decrement in Ergography. B. R. BUGELSKI, Yale University.

In experiments to determine the factors affecting the pattern of work decrement with the finger ergograph, various interpretations of the standard instructions were reported by the subjects. To determine the extent of the influence of instructions, each of 10 practiced subjects worked under 3 different sets of specific instructions in a Mosso ergograph. Each subject produced the following different types of work curves under the 3 conditions: (1) Long high level with an abrupt drop; (2) Long high level with a tapering drop; (3) Long high level with a step-like drop to a long low level.

When the subjects were instructed to lift a weight to a fixed goal repeatedly (Instructions 1), they did so more often than when voluntary changes in the goal (Instructions 2 and 3) were permitted; that is, the original high level was maintained longer in the first type of work curve than in the other two.

Five of the subjects worked under a fourth set of instructions which called for continued work for a specified time. The work produced under this condition indicated a tendency to save effort in the early part of the work period and to make a greater exertion as the work period ended. The tendency to spare effort in favor of time and to increase effort at the end is sometimes observed in ordinary work records.

Subjects frequently reported that some aspects of the instructions were favored over others. In such cases insistence on exactly following the instructions resulted in different types of decrement patterns.

These findings suggest that individual interpretations of the standard instructions, such as a determination to work for a certain length of time or to make all the lifts of an equal height, may account for characteristic patterns of decrement in work among different individuals or in the same individual at different times.

Some Psychological and Physiological Accompaniments of Mental Work. JOSEPH E. BARMACK, College of the City of New York.

The study is divided into 3 parts:

(1) An exploratory study on 15 subjects who operated the Poffenberger Pursuitmeter for 70 minutes. Oxygen consumption ratings on attitudes of interest, attentiveness, feelings of pleasantness, strain, irritation and fatigue, and accuracy records on the pursuitmeter were recorded at ten-minute intervals.

(2) The second experiment consisted of a study on 12 subjects who worked for 90 minutes adding 2 six-place numbers and another 90 minutes taking 5 different forms of the Otis Self-Administering Examinations. The heart rate, systolic and diastolic pressures, the work output and reports of attitudes of interest, attentiveness, feelings of irritation, strain, fatigue, and sleepiness, the estimation of time, and the per cent of time spent in day dreaming were obtained at fifteen-minute intervals.

(3) The third experiment, which was performed on 12 other subjects, was a repetition of (2) using oxygen consumption as the physiological index.

All factors studied tended to vary during work. The metabolic and vascular indices frequently vary independently of work output. The data strongly suggest that the vital activities measured are dependent upon the attitude of the subject toward the work. An attitude of interest results in sustained or rising vital activity whereas an attitude of boredom results in a continuous or sporadic depression of vital activity.

Some paradoxical results were obtained with some subjects, such as high metabolism and low output and vice versa, report of greatest irritability at lowest points of vital activity, etc. The data suggest that there is little systematic difference between so-called "mental

fatigue" and "boredom" for periods of mental work of moderate length. A new theory of mental blocking and of boredom is submitted.

Perceptive Errors in Time Judgments of Behavior. ADELBERT FORD, Lehigh University.

The timing of behavior, by the use of chronometric devices operated by human observers without a knowledge of the conditions for perceptive errors, suggests only too plainly a widespread and common deficiency in the precision of psychological experimentation.

The analysis of conditions for error will be more significant if we first reach a statement of a situation favorable for optimal accuracy. By the procedure of preliminary experimentation, and a review of the literature on reaction time errors, we approached such an optimal situation, and then began measuring the conditions for error, one at a time, according to the rules for the independent control of variables.

With respect to optimal accuracy, we are able to show that it is possible for a human observer to time the duration of a behavior phase, between 1 and 10 seconds duration, with such a degree of precision that the median value of his judgments is less than 2 milliseconds (sigma) from the objective standard. Around this median it has been possible to keep the variability, expressed in terms of the range of the middle 50% of the judgments, as low as ± 17 milliseconds, in a sample of 100 judgments.

With respect to the conditions for error, the optimal rate of movement was 1 minute of the visual angle in a duration of 10 milliseconds; any deviation in speed caused an increase in errors. The minimal duration of time, without serious increase in errors, was .7 second. Nearly all significant judgment was lost at .3 second. Mechanical and human moving members were compared.

A New Psychogalvanometric Technique in Criminal Investigation. W. G. SUMMERS, Fordham University.

The apparatus employed and the technique developed for the distinction of guilt from complicity and innocence have been described in a previous contribution. The present research presents the results of this apparatus and technique as applied to actual criminal cases. The cases included instances of murder, theft, assault, felonious entry and abduction. In some of these actual cases the opportunity was presented for the effective distinction of guilt from

complicity. In other instances, innocent suspects were clearly distinguished from both accomplices and the guilty. In all instances, there was complete success, not only in the individual cases, but in the groups studied. These results were confirmed by confession, judicial procedure or subsequent investigation. The success of the criminal application established conclusively the reliability of the technique which had been previously determined by a long series of experiments.

PROCEEDINGS OF THE WESTERN PSYCHOLOGICAL ASSOCIATION, CLAREMONT, CALIFORNIA, JUNE 18–19, 1937

FRANK C. DAVIS, SECRETARY, UNIVERSITY OF CALIFORNIA AT LOS ANGELES

The Association met at the Claremont Colleges, Claremont, California. Approximately 150 psychologists attended the meetings. Miss Mary B. Eyre was the chairman of local arrangements.

Officers elected for the ensuing year, 1937–1938, were:

President: Paul R. Farnsworth, Stanford University.

Vice-President: Howard R. Taylor, University of Oregon.

Secretary-Treasurer: Frank C. Davis, University of California at Los Angeles.

The Association will meet in June, 1938, at the University of Oregon, Eugene, Oregon.

PROGRAM

Opportunities for Research in a Medical-Psychological Service Center. ELLEN B. SULLIVAN, University of California at Los Angeles.

An account of the development of a psychological clinic in the Children's Hospital, Los Angeles. Cases handled ranged from birth to 12 years of age. From 1925–1937, over 1,000 cases were handled. These included the following groups or types of disorders: epilepsy, spastic paralysis, amnesia and aphasia, allergy, anemia and general metabolic disorders, feeblemindedness, school maladjustment, behavior problems, personality difficulties and emotional disorders, dependent and other placement cases, court cases including adoption cases, and other miscellaneous cases. Some psychological or educational problem was presented by each case, related to or in addition to the physical disability mentioned. The clinic accepted all types of cases referred, in order of reference. After a period of initial sampling of several years, particular groups were more intensively sampled and problems formulated. The problems so formulated

were made possible by the coöperation of the medical staff, working with particular groups. Several studies in progress have had initial publication. An illustration of such a study is that of the convulsive (epileptic) group: a study is being made of the mental traits and behavior manifestations of this group; changes with maturation, treatment, and other physical conditions; conditioning of seizures; educational and social adjustment of the group; and other phases of the disorder. Other studies include the following: the nature of the spastic group and their educational, personal, and social adjustment; certain traits of the allergic group; the aphasias in children; the adjustment of the feebleminded in the home; and other related problems.

Analysis of Euphoric and Depressed Moods in Normal Subjects. WINIFRED BENT JOHNSON, Stanford University.

The Rôle of Emotion in Cases of Special Disability. GRACE M. FERNALD, University of California at Los Angeles.

The Constancy of Mental Test Performance During the Preschool Period. MARJORIE PYLES HONZIK, Institute of Child Welfare, University of California.

A group of 250 children who comprise a representative sample of the children living in Berkeley, California, was tested on 1 of 2 preschool tests (California Preschool Schedule I or II) at specified age levels during their preschool years, and on the Stanford-Binet at the ages of 6 and 7 years. The data have been assembled to show the constancy of mental test performance during this age period.

The constancy of mental test performance between the ages of 21 months and 5 years was found to be essentially the same for the 2 different preschool mental tests, and for 2 different but comparable groups of children (subsamples of the main group). The results show that although test performance is fairly constant over short age periods, mental test constancy appears to be markedly dependent on both the ages of the children when tested and the interval between tests.

Emotional Stability and Political Opinions as Related to Age and Income. RALPH H. GUNDLACH, University of Washington.

A number of adults of various ages and economic status filled out a questionnaire involving items diagnostic of emotional stability, and items concerning political and economic beliefs.

The problem involved an analysis of the effect of the depression

upon opinions and attitudes. The data are analyzed with regard to the income level, major changes in income over the past 6 years, and with regard to the period during which the individual came to maturity—whether war-time, pre-depression time, or depression time.

Factors Influencing the Relative Difficulty of Maze Blinds. CHARLES H. HONZIK, University of California.

For 23 groups of rats trained on a fourteen-blind elevated maze the rank order correlations between difficulty of blinds on the very first run and the difficulty on all succeeding runs ranged from .50 to .92. Groups of rats that differed in the loss of one or another sense, *e.g.* blind, deaf, anosmic, blind-anosmic rats, were found to have closely similar distributions of errors in the various blinds, the rank order correlations between the various groups ranging from .54 to .97. Tests which involved various stimulus changes and which disturbed maze performance were made and the distributions of errors on these tests were also closely similar, correlations ranging from .63 to .92. Finally, the distributions of errors on the tests correlated fairly highly with distributions on runs preceding the tests. It is concluded that the relative difficulty of blinds is independent of exteroceptive stimulation and that the effective factors are intra-organic.

The Effect on Performance of the Introduction and Removal of a Delay Within the Maze. JOSEPH B. COOPER, University of California at Los Angeles. (Introduced by H. C. Gilhousen.)

The temporal effect of introduction and removal of intermediate "goals" was investigated in the white rat. The technic used was that of delaying the animals at the mid-point in a typical maze situation. One group was delayed *after* the training period, while the second group was delayed *during* the training period. The running speed was tested before and after the reverse situation was introduced to each group. In the case of the first group, speed decreased immediately in that part of the maze lying before the delay point; in the second group a marked increase in speed was noted in that same part immediately. The animals seemed to make rapid and significant adjustments to intermediate goals.

A Preliminary Analysis of "Anticipatory" Action in Mazes. H. C. GILHOUSEN, University of California at Los Angeles.

Detention of 60 seconds in the middle of a maze slows down speed of running up to the detention box. The animals do not adapt to

such delay. Removal of the detention is related to an immediate drop in running time to a basic rate. The animals retain without decrement the effects of removal of a 60 second delay for 3 minutes and possibly longer. Animals trained on 2 different mazes under detention conditions in both, upon release from detention conditions in the first maze, show marked increase in speed of running in that maze. There is little evidence that the animals anticipate lack of detention in the second maze.

A Quantitative Study of Copulatory Frequency in Male Rats Following Castration. CALVIN P. STONE, Stanford University.

Male rats castrated after puberty retain copulatory ability for various periods of time thereafter. A previous study of this function on 45 males castrated at the age of 90 days showed that 33% ceased to copulate during the first month after castration; 74% by the end of the fourth month; and 94% by the end of the sixth. In these tests no quantitative tests of copulatory frequency were made.

In the present experiment male rats 10 months of age are being studied. Each test for copulatory frequency consists of 2 fifteen-minute subtests given with an interval of 20 minutes between the subtests. The tests are given at six-day intervals. The experimental group was castrated on the day following the first standard test. Test results for subsequent tests are compared both with the results from the first test and with the results from controls similarly tested. The drop in frequency of copulation in the first test after castration is approximately 50%; the same relationship holds for ejaculations. At the second test, frequency of copulations has dropped to approximately 25% of the original and ejaculations to approximately 10% of that in the first test. Similar relationships hold when the castrates are compared with the normal controls. In the third, fourth and fifth tests, most of the animals are inactive; low frequencies are recorded for those that are still active.

Measurements of Learning and of Behavior Under Varying Conditions of Motivation. MERLE HUGH ELLIOTT, Oakland Public Schools, Oakland, California.

The data here reported were obtained in an attempt to investigate the effects of definite motivational conditions upon behavior as well as upon learning. Behavioral measurements included speed of running, "idle" time, and 2 indices of activity. The motivational conditions for different groups of animals were 12, 24, 36, 48, and 72

hours of food deprivation. The reward was also varied quantitatively and qualitatively.

The data indicate the relative effects of these different motivational conditions upon behavior at various stages of learning and also their effects upon the learning process itself.

The Effect of Early Inanition upon Maze Learning in the Albino Rat. WILLIAM C. BIEL, Stanford University.

A group of white rats (N=26) subjected to severe inanition from birth was compared in maze learning to 3 groups of control animals (N= 30 in each group) which had been raised normally until the period of maze training began. Between 36 and 55 days of age the animals were trained on the Warden U-maze with 2 trials per day and with food as the incentive. The 3 control groups were motivated to different degrees by controlling their weights in different ways throughout the experiment. Between 61 and 90 days of age the animals were trained on the Stone multiple-T maze with 1 trial per day and with water as the incentive.

The results from error and time scores on the 2 mazes show that severe inanition from birth causes no deleterious effect upon maze learning ability.

A Statistical Mechanics for Sociology. ROBERT D. GORDON, Scripps Institution of Oceanography, La Jolla, Calif. (Introduced by Harry Steinmetz.)

The problem of interpreting certain data of the commercial sardine fishery suggested a numerical analysis of the schooling behavior of the fish. Numerical postulates are presented for consideration which, it is believed, correspond fairly well to the physical and psychological principles which seem to be operative in the schooling behavior. It is the author's intention to develop further improvements for these postulates before attempting to construct a mathematical theory on their basis. A more precise knowledge of the operation of Gestalt principles, in the perception by the fish of companions against different backgrounds, seems to be needed.

Is Epiphenomenalism Dead? JOHN W. TODD, University of Southern California.

A study of the psychological literature of the last 5 years reveals a healthy mentalism still running through its pages although, during the same period, the objective experimentalists for the most part

have found no place for it. While it is conceded that psychological concepts must lag behind the discoveries of the laboratory, they appear unnecessarily tardy in catching up. In certain instances, they are a quarter-century overdue. The lively infusion of various types of European psychology and psychiatry, aided somewhat by our own so-called eclectics, has played a part in keeping mentalism alive. Also, certain of its outstanding enemies seemingly have become less militant or have turned to something else, while our own contemporary writers in the fields of psychiatry and the abnormal appear to find little that is objectionable in it. However, it may be said that all those who find mentalistic concepts convenient are not necessarily epiphenomenalists.

Adaptation of the Galvanic Response to Auditory Stimuli. CLYDE H. COOMBS. (Introduced by Warner Brown.)

Auditory stimuli were presented to 187 women college students and changes in the electrical resistance of the skin were recorded. Startle and affectivity ratings were secured on all stimuli and the data analyzed from the point of view of response decrement. Conclusions:

1. Galvanic adaptation takes place more rapidly with a fifteen-second interval than with a thirty-second interval between stimuli.

2. A relationship is established between the mean startle rating of a stimulus, its mean deflection, and its absolute response decrement.

3. A relationship is established between the relative mean startle decrement of a stimulus and the relative response decrement.

4. Evidence is offered that galvanic adaptation to auditory stimuli is in part general and in part specific to the stimulus. An hypothesis is presented as to the nature of the factors underlying transfer of adaptation.

The Effect of Injections of Benzedrine on the Activity of White Rats. LLOYD W. SEARLE and CLARENCE W. BROWN, University of California.

Fifteen male rats were given subcutaneous injections of benzedrine, every 4 days. Days of rest were inserted before and after each drug day and a control injection of normal saline was given between the rest days. Activity was measured by means of revolving cages, recordings being taken every hour from the time of injection at 9:00 A.M. until the time of feeding at 5:00 P.M. Water was present in the cages at all times; food was placed in the cages after each

day's run. The animals were allowed 21 days in which to become accustomed to the cages before injections were begun. An uncorrected split-half reliability coefficient, computed on the ten-day period from the ninth to the eighteenth day, was $.97 \pm .015$. A marked increase in activity was noted after each injection of benzedrine. The increase was greatest for the first hour; then it gradually decreased, disappearing at the end of about 4 hours.

An Investigation of Basic Skin Resistance Levels During Sleep Under Differing Conditions. DAVID R. CRAIG, Stanford University. (Introduced by C. P. Stone.)

Basic skin resistance levels of a group of college students were recorded over a period of 30 minutes on each of 2 nights. The subjects were instructed to attempt to go to sleep at the beginning of each period. Whether or not they were successful was determined by their ability to recall certain signals given them during the experimental period. On one night the room was completely dark and relatively soundless. On the other night the conditions included a flashing light throughout the period and the ticking of an alarm clock. The results show a tendency for the basic skin resistance level to rise during sleep, although the difference between the sleep and the non-sleep groups was not statistically significant. This lack of a significant difference was coupled with evidence that the direction of the change during sleep was not the same for all subjects, and that the degree of change was not necessarily related to the sleeping condition. Dreaming was probably an important factor in the direction and extent of the change. Contrary to expectation, it was found that a greater percentage of subjects slept under the "disturbing" conditions while a greater percentage dreamed under the "quiet" conditions.

The Reliability and Validity of Some Indices of Changes in Skin Resistance as a Measure of Affectivity. NATHAN W. SHOCK and CLYDE H. COOMBS, University of California.

The purpose of the study is to select the measure of the galvanic response which will show the maximal: (1) reliability; (2) internal consistency; (3) physiological significance; and will have a frequency distribution of individual scores approximating normality. The data used are photographic records of simultaneous changes in apparent skin resistance of right and left hand from a group of 100 adolescents (50 boys and 50 girls, ages 13.5 to 16) to whom 18 odors

were presented on 2 successive days. The following indices of galvanic activity have been completed and compared:

(1) Absolute resistance change in ohms.
(2) Conductance change in ohms.
(3) Relative resistance change in ohms.
(4) Log ohms resistance change.
(5) Log conductance change.
(6) Log of relative resistance change.
(7) Log of ratio of initial to maximal displacement in resistance level.
(8) Rate of resistance change in ohms per second.

Tentatively, the conclusion is reached that log conductance is the best of the above indices on the basis of all the above criteria.

The Concept of Reinforcement as Used in Conditioning Experiments. ERNEST R. HILGARD, Stanford University.

The two classes of conditioned response as suggested by Konorski and Miller and by Skinner raise important questions for theory. The usual type of conditioned response, which may be described as fractional component substitution, is reinforced by behavior in some respects similar to the conditioned response. The other type, the instrumental variety, is reinforced by markedly different behavior, commonly conceived of as a goal-reaction in relation to reward or punishment. The problem arises whether one type is fundamental and the other a derivative from it. Tentative answers only can be given because of the lack of precise experimental analyses of some forms of conditioning. The present points of view of Guthrie, Hull, Thorndike, and Tolman with respect to the problem are characterized.

Spaced Practice in a Motor Skill and the Stimulation-Maturation Hypothesis. LEON R. DORÉ, Stanford University. (Introduced by E. R. Hilgard.)

The stimulation-maturation hypothesis, inferred in the writings of Wheeler and of Snoddy, proposes that growth is induced by stimulation at a rate characteristic of the organism at its level of age-maturation. Granted the proper minimum of stimulation, improvement in performance should be more closely correlated with elapsed time than with the total amount of practice. This hypothesis is tested in experiments with the Koerth pursuit rotor, involving different distributions of practice and rest. The advantages of spacing over massing are considerable, as the hypothesis would predict. On the other hand, internal evidence shows that fatigue is such an

important factor in the disadvantages of massing that the stimulation-maturation theory may be superfluous. No evidence is found for the 2 processes which Snoddy identifies as primary and secondary growth.

The Effect of Instructions on Conditioned Discrimination. LLOYD G. HUMPHREYS, Stanford University. (Introduced by E. R. Hilgard.)

Two differing voluntary sets were superimposed on visual conditioned discrimination. On the first day, simple conditioned responses were established to an illumination change followed by a puff of air to one cornea. Another visual stimulus, never followed by the air, was added on the second and third days. One group was instructed to wink to the positive stimulus and not to the negative; another group had the opposite instructions. Discrimination is virtually complete from the start of the second day in the former. In the latter, the conditioned response to the positive light gradually gains in strength relative to the strength of the opposed voluntary set. These groups are compared to 2 others having differing amounts of knowledge of the light-puff relationship, the group having complete knowledge showing more discrimination initially. The difference disappears, however, when the probabilities of the situation become established.

On the Hypothesis that Reduction in Nystagmus Following Repeated Elicitation Is in the Nature of Learning. FRANKLIN FEARING, University of California at Los Angeles.

Experiments Toward a Metric for Mental Functions. J. A. GENGERELLI, University of California at Los Angeles.

A method of binomial analysis is proposed whereby the distribution of raw scores on a given psychological function may be reduced to certain operationally defined units—termed α-units—which are comparable for several psychological processes. The development of this unit makes it possible to look upon the variability of a given test as generated by a number of equally weighted independent determiners which are 1 less than the number of α-units to which the range of raw scores has been reduced.

Experiments are reported in which the above method is employed. The experiments are in the nature of tests of 3 deductions which may be made on the basis of the postulates underlying the binomial technique. The results are in definite conformity with the demands

of the hypothesis. Among other things, it is shown that in such a relatively simple learning task as code transcription involving but 4 elements, continued practice tends to diminish variability in terms of α-units. It is also indicated that the method of binomial analysis does not apply unless the raw scores constituting the data are capable of a definite quantitative meaning.

A Rational Theory of Discrimination Learning. HAROLD GULLIKSEN and DAEL L. WOLFLE, University of Chicago. (By Invitation.)

A rational theory is developed which describes the course of discrimination learning and predicts the nature and accuracy of transfer to new stimuli.

The strength of a response tendency for a given configuration i is assumed to be a function of 2 factors:

(1) *Initial Strength:* Before any practice has been given the strength may vary due to position preferences, brightness preferences, etc.

(2) *Practice:* Each reward increases the strength of a response tendency and each punishment decreases it. The practice of a response to any configuration j, affects the strength of the same response tendency to configuration i according to a diminishing function of the distance between i and j.

Some of the deductions regarding the ease or difficulty of learning different discrimination problems, the nature and extent of transfer, the derivation of the learning equation, and the reasoning leading to some of the deductions will be presented.

The Psychophysics of Mental Test Difficulty. J. P. GUILFORD, University of Nebraska. (By Invitation.)

By the use of the Seashore tests of Pitch Discrimination, Intensity Discrimination, Time Discrimination, and the test of Tonal Memory, it is shown that the easiness of an item, as determined by absolute scaling methods, is proportional to the logarithm of the magnitude of the stimulus. It is proposed that this is a case of Fechner's universal psychophysical law. From this result it may follow that difficult items draw upon different primary abilities from easy ones. It is suggested that the method of absolute scaling be adopted as an additional psychophysical method. The computation of traditional thresholds was demonstrated. It is urged that the unit of increment of difficulty be adopted as a very generally applicable unit of psychological measurement and that stimulus magnitudes be more generally given in terms of logarithmic scales.

Responses of Isolated Roller Canaries. MILTON METFESSEL, University of Southern California. (Address of the Retiring President of the Western Psychological Association.)

The Present Use of Intelligence Tests in Industry. LUCILLE K. RHODES, University of Southern California. (Introduced by J. W. Todd.)

The purpose of this survey is to determine the current opinion of those connected with industries on the usefulness of intelligence tests as instruments of guidance. It seemed desirable to know to what extent confidence in standard tests has developed among industrialists in the United States. Questionnaires covering the application of the tests, the extent to which the tests are being used, and the results which have been achieved, were mailed to various industries throughout the United States. In particular, the industries selected were those cited by Dr. Douglas Fryer in his article, "Intelligence Tests in Industry," in the *Personnel Journal,* 1935, 13, 321, 323, as then using the standard tests or having formerly used them. The results are tabulated as to types of tests and their relative popularity.

NoteScript and NoteTyping: Their Nature and Economy. J. E. COOVER, Stanford University.

NoteScript is a legible system of condensed writing. Its alphabetic elements are characteristic parts of our familiar script letters; consequently it is at once easy to read and write. As written, it follows the phonetics of oral speech, and saves 70% over longhand: 1 line of NoteScript contains over 2 lines of pica typing.

NoteTyping is a legible system of condensed typing. The phonetic system of NoteScript is transferred to the keyboard of the typewriter. So far as the alphabet extends the regular letters take the place of the NoteScript elements and retain their phonetic values. NoteScript extensions of the alphabet are represented by appropriate symbols on the keyboard. The legibility of NoteTyping is equal to that of NoteScript, and it saves 40% over regular typing.

These systems illustrate the application of psychological principles to the problem of recording speech legibly and economically by either hand or machine.

The Association-Motor Technic for Studying Mental Conflict. JOHN W. GARDNER, University of California. (Introduced by Warner Brown.)

The validity of Luria's association-motor technic as a means of studying mental conflict was investigated quantitatively. Disturb-

ances in the pressure records of the sort described by Luria were observed, and a statistically significant relationship between certain of these disturbances and the critical words in the word-association test was demonstrated.

Motivation Through Punishment. WARNER BROWN, University of California.

At the Los Angeles meeting in June, 1935, an experiment was reported on human learning in a mechanical maze. The results indicated (but inconclusively) that subjects who received more punishment during learning made better records than those who received less punishment.

The present report covers a continuation of the same experiment. Groups are compared: (a) control, *no shock;* (b) shock on *correct, i.e.* one shock at each choice-point; (c) shock on the *first* key touched at each choice-point, *i.e.* one shock at each choice-point without regard to correctness. The 1935 data contain a group just like (b), shock on *correct,* and another comparable group punished in the conventional way on *errors,* as well as a group shocked on errors *and also* on half the correct responses.

Data on the Chord-Tone-Resonance Theory of Vowels. PAUL R. FARNSWORTH, Stanford University.

Harvey Fletcher states that the vowels o͞o, o͝o, ō, ô, u͝, and ä each have 1 major formant while the ă, ĕ, û, ā, ĭ, and ē have 2 equally intense ones. In the present study these formants were presented to subjects who were asked to look for resemblances to the above mentioned vowels. While extremely consistent data resulted, the medians and modes of the vowel ballots did not invariably fall on the Fletcher overtone placements. The relation was relative, not absolute. It is the pitch *rank* of the placements which is closely related to the rank of the vowel sounds (rho of .98). Vowels possessing both high and low strong overtone bands were mentioned when high pitches were played; those with only strong low bands when low pitches were played. When the subjects were not told what vowels might be used, many ballots appeared for ī.

Intercorrelations Among Tests of Learning and Memory. R. W. HUSBAND, University of Wisconsin. (By Invitation.)

One hundred college students have been tested individually in 3 one-hour periods separated by a week each on 17 different tests of

learning and memory. These tests ranged from simple sensory and motor tests to very difficult complex intellectual functions. We were interested in studying the intercorrelations to see: (1) the general nature of the intercorrelations; (2) the correlations within various classes of functions, to discover any possible general or group factors, such as are claimed to exist in intelligence. Ninety-one intercorrelations will be presented.

Reaction Time of Brake Application to Visual and Auditory Stimuli. C. M. LOUTTIT, University of Indiana. (By Invitation.)

Approximately 600 men and 100 women attending the Indiana State Fair voluntarily acted as subjects in the measurement of reaction time for moving the right foot from the depressed accelerator to applying the brakes on a Ford V-8 in response to visual (red light) and auditory (auto horn) stimuli. Three trials were given in each modality. The range of individual differences for men and for women and for each sense modality have been compared and the significance of the various differences between them determined. Comparisons were also made between rural and urban groups and various occupational classifications. Relationships between reaction times for each sense modality and between years of driving experience and chronological age were determined by correlational analysis.

The Effect of Auditory Stimulation upon the Visual Perception of Brightness. JUDSON S. BROWN, University of California at Los Angeles. (Introduced by F. C. Davis.)

A New Type Instrument for Polygraphic Recording. (Demonstration.) D. P. WILSON, Consulting Psychologist, Los Angeles.

Research was begun 7 years ago on a more precise instrument especially adapted to certain experimental situations in physiology, medicine, endocrinology, and in deception work, which require immediate and visible recordings for constant comparison throughout the experiment. Two terms are introduced, *Psychography,* referring specifically to polygraph records in deception work; and *Psychographer,* the interpreter of such forensic records. Comparisons are made with Spectrography as a forensic science. Two major difficulties in making and defending Psychography as a science, lie, first, in standardization of the instrument, and, second, in standardization of the interpretations. Toward both these ends an all-electrical recording instrument is being completed which will give uniform

precise records for comparison, accurate to tenths of a unit, and subject to amplification and control. The instrument is designed for 5 interchangeable pickup measures. Final description and specifications will be ready for publication this fall. A portable polygraph will be on exhibit, as well as specimen deception records of criminals, and typical samples chosen from 200 laboratory "lies" created as an experiment in Psychography.

Absolute Change and Relative Stability in Vocabulary Test Scores of College Students. HOWARD R. TAYLOR, University of Oregon.

According to Dean McConn (*North American Review,* 1931), "the effect of college on the word supply of the ordinary student appears to be almost negative and in some cases positively injurious." The average gain of 50 University of Oregon students who had taken the Inglis Test of English Vocabulary Form B in 1927 and were retested with both Form B and Form A of the same test in 1931 was 19 words per student, an addition of ⅕ to their *general* vocabulary. Due to lack of "ceiling" in the examination the correlation between gain and initial score was $r=-.8$, yet the correlation between the initial test in 1927 and a retest with an equivalent form in 1931 was $r=.88$. Thus, in spite of the fact that as measured by this test those who knew the fewest words at entrance to college gained the most, their relative standing in knowledge of word meanings does not change materially. Further statistical analysis and the implications of the results with reference to the development of word meanings and the psychological measurement of it are presented.

A Correlational Analysis of Factors in Speed of Reading Tests. LEE STOCKFORD, JR., University of Southern California. (Introduced by R. H. Seashore.)

A Test of General Ability in the Fiji Islands. CECIL W. MANN, Sydney, Australia. (Introduced by P. R. Farnsworth.)

An attempt was made to measure the general ability of the Fijians and the Indians in the Colony of Fiji. A battery of non-verbal tests was prepared using completions, classifications, similars, opposites, analogies, number series and digit-symbol substitution. Correlations between teachers' estimates and the Fiji Test varied from 0.68 to 0.89; between the Otis Test and the Fiji Test (applied to Anglo-Saxon children) the coefficient of correlation was 0.74 ± 0.02; between the Goodenough Test and the Fiji Test, the coefficient of

correlation was 0.75 ± 0.032. A retest in 2 schools over a time interval of 3 months gave a reliability coefficient of 0.89 ± 0.032, and by the split-halves method a reliability coefficient of 0.75 ± 0.021. The differences in mean scores of the 2 races at different age levels are statistically significant, but there are sociological factors which indicate that racial comparisons are not reliable. The main value of the test will be for the purpose of internal grading of the separate races.

PROCEEDINGS OF THE THIRTY-SECOND ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

FRANK A. GELDARD, SECRETARY, UNIVERSITY OF VIRGINIA

The Thirty-second Annual Meeting of the Southern Society for Philosophy and Psychology was held in Columbia, South Carolina, on March 26 and 27, 1937. The University of South Carolina was the host institution. Two sessions devoted to contributed papers in psychology were held in Drayton Hall of the University on Friday; two philosophy sessions were held in University Chapel on the same day. The Saturday morning session, in Drayton Hall, was a joint one. Thirty-four papers were read, 19 being in psychology, 15 in philosophy.

The Council of the Society met Thursday, March 25, at 8:00 P.M., in the Hotel Columbia, President Lyle H. Lanier presiding. Other Council members present were: A. G. A. Balz, F. A. Geldard, C. P. Heinlein, H. M. Johnson, F. Marti, H. C. Sanborn, and M. ten Hoor.

The annual banquet took place at the Hotel Columbia on Friday evening, March 26. An address of welcome was made by President McKissick of the University of South Carolina. The presidential address, *The Descriptive Categories of Psychology,* was delivered by Dr. Lyle H. Lanier.

The annual business meeting of the Society was held on Saturday, March 27, convening at 12:15 P.M. with President Lanier in the chair.

MINUTES OF THE ANNUAL BUSINESS MEETING

The minutes of the Thirty-first Annual Meeting, published in the PSYCHOLOGICAL BULLETIN, Vol. 33, No. 8, October, 1936, were approved as printed.

The secretary's report was read and approved. It contained an analysis of membership, a statement concerning the decision on the part of the Council to hold the 1937 meeting in Columbia, and an account of the selection of papers for the Columbia meeting. Membership at the time of the previous meeting numbered 193. Fourteen

members having been lost through resignation and death, 34 having been added throughout the year, the official membership list contained 213 names.

The treasurer's report was read and accepted. Assets and receipts totaled $891.87, expenses $195.26, leaving a cash balance at the close of the fiscal year, March 20, 1937, of $696.61. This represents a net gain during the year of $95.26, the cash balance on April 11, 1936, having been $601.35.

No formal committee reports were made, though it was announced that the instructions of the 1936 meeting to the Committee on the Teaching of Philosophy in the South had been complied with, copies of that Committee's report having been distributed to various southern colleges. An informal report was made by Professor Nafe on the status of the work of the Committee on the First Course in Psychology.

The Council of the Society submitted a recommendation: "That two standing committees be created, consisting of three members each and appointed by the President, one to be known as the Committee on Philosophy, the other the Committee on Psychology. These committees are to receive no special instructions but would be prepared to consider such matters as might arise related to the teaching of their respective subjects. They would, in a sense, continue the work of the two former temporary committees but would also consider other matters involving the general academic status of these subjects in the South." On motion of Professor Johnson the recommendation was adopted.

On recommendation of the Council, the following new members were elected:

Moselle Ashford, State Teacher's College, Florence, Ala.
Kenneth K. Berry, University of Virginia.
Connie Jones Burwell, University of North Carolina.
Edgar M. Cook, Carson-Newman College.
Thomas J. Eskridge, Jr., Duke University.
Leonard J. Eslick, University of Virginia.
Iredell Jenkins, University of Virginia.
William McGehee, North Carolina State College.
Robert D. Miller, Florida State College for Women.
George T. Preer, University of Virginia.
Carl B. Watson, Duke University.
Karl E. Zener, Duke University.

On recommendation of the Council, the following were elected: President, Marten ten Hoor, Tulane University; Council Members:

Emily S. Dexter (1937–1938), Agnes Scott College, Marjorie S. Harris (1937–1940), Randolph-Macon Woman's College, and John Paul Nafe (1937–1940), Washington University.

The secretary announced that invitations had been received to hold the 1938 meeting in: New Orleans (Tulane University), Charlottesville (University of Virginia), Baton Rouge (Louisiana State University), St. Louis (Washington University). Two additional invitations were extended from the floor, that of Duke University and that of the University of Tennessee. After considerable discussion it was voted to continue the policy of recent years and hold the 1938 meeting in a central location. Accordingly the invitation of the University of Tennessee to come to Knoxville was accepted.

On motion of Professor Liddell the Society gave a rising vote of thanks to the University of South Carolina for its cordial hospitality.

It was announced by the Secretary that the Council had accepted the generous offer of Mr. Zed H. Burns of Birmingham, Alabama, to provide printed copies of a handbook to incorporate the list of members and constitution of the Society.

PROGRAM

I. Psychology Papers

The Curve of Forgetting and the Distribution of Practice. HOWARD EASLEY, Duke University.

Development of Thought. BUFORD JOHNSON, Johns Hopkins University.

Experimental Studies of Skin Sensitivity to Mechanical and Electrical Vibration. B. VON HALLER GILMER, Carnegie Institute of Technology.

Effects of Asphyxia Block upon Vascular Action and upon Sensitivity to Cold and Warmth. JOHN P. NAFE, Washington University, St. Louis, Mo.

An Active Principle in Binocular Interdependence. S. RAINS WALLACE, University of Virginia and Ohio State University.

The Systematic Classification of the Concepts of Conscious, Subconscious, and Unconscious. J. B. MINER, University of Kentucky.

Learning and Retention in Educational Broadcasting. STEUART H. BRITT, George Washington University, Washington, D. C.

A Preliminary Report on a Recently Developed Eye-Hand Coördination Test. JOSEPH E. MOORE, George Peabody College.

The Varying Function of Affectively Toned Olfactory, Visual, and Auditory Cues in Recall. MARGARET M. RATLIFF, University of Kentucky.

The Effect of Phenobarbital upon the Maze Learning of White Rats Reared upon Rich, Normal, and Vitamin A and Vitamin D Deficient Diets. CLIFFORD T. MORGAN, University of Rochester, and PAUL E. FIELDS, Ohio Wesleyan University.

The Effects of Inactivity Produced by Sodium Amytal on the Retention of the Maze Habit in the Albino Rat. R. W. RUSSELL, George Peabody College.

The Modification of Conditioned Eyelid Responses by Non-Reinforced Elicitation. JAMES M. PORTER, JR.

A Technique for Rapid Conditioning of Animals. NORMAN L. MUNN, George Peabody College.

Delayed Reaction by an Infant Chimpanzee Under Unusual Conditions of Motivation. T. L. MCCULLOCH, Yale Laboratories of Primate Biology.

Conceptions of Drive (as Urge and as Internal Stimulus) in Relation to the Conditional Salivary Response to Food and Acid. KARL ZENER, Duke University.

An Experimental Evaluation of Patterned Responses. HENRY F. DICKENSON, Lincoln Memorial University.

Statistical Abuses in Experimental Psychology. C. PAUL HEINLEIN, Florida State College for Women.

The Genesis of Action Preferences. G. R. WENDT, University of Virginia.

The Elasticity of the Concept "Specific Response." H. M. JOHNSON, National Research Council.

II. Philosophy Papers

God and the Cosmos in the Philosophy of Nicholas of Cusa. ANNA FORBES LIDDELL, Florida State College for Women.

Definition and Discovery in Plato's "The Sophist." G. THOMAS PREER, University of Virginia.

Science and Morals. J. B. GITTLER, University of Georgia.

The Subjective Basis for the Judgment of the Beautiful. IREDELL JENKINS, University of Virginia.

Value in Whitehead's Metaphysics. LEWIS M. HAMMOND, University of Virginia.

An Examination of Stern's Philosophy. HERBERT C. SANBORN, Vanderbilt University.

The Theory of Types and the "De Ente." KENNETH K. BERRY, University of Virginia.

Three Levels in the Analysis of the One and the Many in St. Thomas Aquinas. LEONARD J. ESLICK, University of Virginia.

Kant's Theory of Intuitus Intellectualis in the Inaugural Dissertation of 1770. CHRISTOPHER B. GARNETT, George Washington University, Washington, D. C.

The Rôle of Graphical Methods of Analysis in the History of Logic. CHARLES K. DAVENPORT, University of Virginia.

A Re-Statement of the Question of Being. HOMER G. RICHEY, University of Virginia.

The Refutation of Indeterminacy. PETER A. CARMICHAEL, Louisiana State University.

On the Sense of the Finite. MARJORIE S. HARRIS, Randolph-Macon Woman's College.

Mediaevalism in Modern Political Thought. MARTEN TEN HOOR, Tulane University.

The Indefensibility of Dictatorship. A. G. A. BALZ, University of Virginia.

PROCEEDINGS OF THE TWELFTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

ARTHUR G. BILLS, SECRETARY, UNIVERSITY OF CINCINNATI

The Twelfth Annual Meeting of the Midwestern Psychological Association was held May 7 and 8, 1937, at the University of Illinois, under the Presidency of Dr. Harvey A. Carr of Chicago University. There were 373 persons who registered attendance.

At the business meeting, it was voted to take the necessary steps to affiliate with the American Psychological Association. It was voted that in the future the annual meeting date should be the week of May 1, or within one week of it, and would not be subject to change because of conflict with other organizations. A committee was appointed, with A. R. Gilliland as chairman, to investigate the question of publication of the abstracts of program papers in full. Fifty-three new members were elected, of whom 33 were members or associates of the American Psychological Association. A letter was read by H. B. English explaining the work of the Spanish Relief Committee. The council voted to hold the 1938 meeting at the University of Wisconsin, Madison, Wisconsin. The following newly elected officers of the Association were announced:

President, 1937–1938: Arthur G. Bills, Universtiy of Cincinnati.

Secretary-Treasurer, 1937–1940: J. P. Guilford, University of Nebraska.

Council Member, 1937–1940: E. S. Conklin, University of Indiana.

Council Member to complete J. P. Guilford's unexpired term, 1937–1939: L. E. Travis, University of Iowa.

PROGRAM

FRIDAY MORNING, MAY 7

SESSION A. PHYSIOLOGICAL PSYCHOLOGY

CHAIRMAN, R. C. DAVIS, INDIANA UNIVERSITY

Autonomic Activity and Autonomic Balance in Relation to Personality. R. P. DARLING, Lincoln, Illinois.

The Postural Accompaniments of the Voluntary Inhibition of Micturition. G. L. FREEMAN, Northwestern University.

Evidence of Cerebro-cerebellar Function as Revealed in the Experimental Modification of Behavior in Pigeons. WARD HALSTEAD and GEORGE YACORZYNSKI, Otho S. A. Sprague Memorial Institute.

Applications of Apparatus for Continuous Records of Systolic and Diastolic Blood Pressure. M. L. PHILLIP and C. W. DARROW, Institute for Juvenile Research.

Fatigue in Terms of the Electrodermal Response. C. A. RUCKMICK, University of Iowa.

SESSION B. TESTS AND MEASUREMENTS

CHAIRMAN, C. S. YOAKUM, UNIVERSITY OF MICHIGAN

A Critical Examination of the Otis S-A and Minnesota Clerical Tests. H. A. COPELAND, Cincinnati Employment Center.

Speed and Accuracy in Intelligence. R. W. HUSBAND, University of Wisconsin.

A Diagnostic Test of Typing and Stenographic Proficiency. HARRIETT LANGE, Jewish Vocational Service, Chicago.

A Study of the Relation Between Attitudes, Educational Status, Implicit Verbal Responses, and Accident Susceptibility. A. R. LAUER, Iowa State College.

Test Scores and Their Interpretation. W. A. LURIE, Jewish Vocational Service, Chicago.

SESSION C. GENERAL PSYCHOLOGY

CHAIRMAN, J. E. EVANS, IOWA STATE COLLEGE

Study of Psychological Misconceptions and Fallacies of College Freshmen About to Begin the Study of Human Behavior. H. J. ARNOLD, Wittenberg College.

The Reorganization of Introductory Psychology from the Standpoint of Selecting Material of Most Significance and from the Standpoint of Individualizing and Functionalizing the Teaching of Psychology. ROBERT LEEPER, Cornell College.

An Experiment in the Group Counseling of Low Ranking Freshmen. C. C. Ross, University of Kentucky.

The Measurement and Analysis of Individual Differences in Size of English Vocabularies. R. H. SEASHORE and LOIS ECKERSON, University of Southern California.

The Relation Between the Intelligence of Mothers and of Their Children Living in Foster Homes. DONALD SNYGG, University of Toronto.

FRIDAY AFTERNOON, MAY 7

SESSION A. PERSONALITY

CHAIRMAN, HELEN KOCH, UNIVERSITY OF CHICAGO

Professed Attitudes and Actual Behavior. STEPHEN COREY, University of Wisconsin.

An Interpretation of the Relationship Between Academic Achievement and Eight Personality Scores. NEAL DROUGHT, University of Wisconsin.

Study of the Persistency of an Attitude over a Five-Month Period When Measured by Indirect Methods. V. E. HERRICK, Milwaukee Day School.

The Nosology of Children's Behavior Problems. C. M. LOUTTIT, Indiana University.

Comparison of College Student Leaders and Non-Leaders in Personality. FRED MCKINNEY, University of Missouri.

Thurstone Personality Schedule: A Short Form for High-School Pupils. H. H. REMMERS, LAURENCE WHISLER, and VICTOR DAWALD, Purdue University and Peru High School.

SESSION B. ELECTRO-PHYSIOLOGY

CHAIRMAN, D. B. LINDSLEY, WESTERN RESERVE UNIVERSITY

The Stability and Adaptation of the Human Brain Rhythm. B. K. BAGCHI, University of Iowa.

The Response of the Sensory-Motor Cortex to Peripheral Stimulation. S. H. BARTLEY and PETER HEINBECKER, Washington University School of Medicine, St. Louis.

Variation of Blood Pressure with Skeletal Muscle Tension and Relaxation. EDMUND JACOBSON, University of Chicago.

The Distribution of Muscular Action Potentials. W. A. SHAW, Indiana University.

Brain Potential Studies of Perseveration. L. E. TRAVIS and J. R. KNOTT, University of Iowa.

SESSION C. HUMAN LEARNING

CHAIRMAN, V. A. C. HENMON, UNIVERSITY OF WISCONSIN

The Reliability of the Identification of the Human Voice. FRANCES MCGEHEE, Wichita Child Research Laboratory.

The Effect of Tension upon Retroactive Inhibition and Reinstatement. J. C. STAUFFACHER, University of Chicago.

A Study of Backward and Remote Forward Association in Serial Learning. MARGERY H. TROWBRIDGE, Madison, Wisconsin.

Maturation Levels in the Interests of Elementary-School Children. P. A. WITTY, Northwestern University.

The Effect of Practice upon Groups of Different Initial Ability. HERBERT WOODROW, University of Illinois.

FRIDAY EVENING SYMPOSIA

Personality Measurement: J. P. GUILFORD, University of Nebraska, Chairman.

Psychology of Social Control: H. H. REMMERS, Purdue University, Chairman.

Physiological Aspects of Psychopathology: C. W. DARROW, Institute for Juvenile Research, Chairman.

Joint Symposium on Transfer in Learning and Retention: A. W. MELTON, University of Missouri, and MARION E. BUNCH, Washington University, Chairmen.

Motivation: P. T. YOUNG, University of Illinois, Chairman.

Action-Current Techniques: L. E. TRAVIS, University of Iowa, Chairman.

Theories of Conditioning: ST. CLAIR A. SWITZER, Miami University, Chairman.

Child Psychology: J. E. ANDERSON, University of Minnesota, Chairman.

Reorganization of the First Course in Psychology: R. LEEPER, Cornell College, Chairman.

SATURDAY MORNING, MAY 8

SESSION A. CLINICAL AND SOCIAL PSYCHOLOGY

CHAIRMAN, M. L. REYMERT, MOOSEHEART LABORATORY FOR CHILD RESEARCH

Domination and Integration in the Social Behavior of Young Children. H. H. ANDERSON, University of Illinois.

A Personality Schedule for Family Disphoria. E. S. CONKLIN, Indiana University.

Evidence for a Law of Collective Judgment. HERBERT GURNEE, Western Reserve University.

Some Objective Criteria for the Differentiation of Social Adjustment. J. E. JANNEY, Western College.

Success of Various Occupational Groups in Estimating Public Opinion. R. J. LEWINSKI and N. C. MEIER, University of Iowa.

A Study of Delinquent Boys on the Basis of the Rorschach Test. HERBERT SHUEY, State Industrial School, Topeka, Kansas.

Marital Similarity in Socio-Economic Attitudes. Ross STAGNER, University of Akron.

SESSION B. ANIMAL LEARNING

CHAIRMAN, NORMAN R. F. MAIER, UNIVERSITY OF MICHIGAN

An Experimental Study of Maze Learning in White Rats after Varying Periods of Wakefulness. M. E. BUNCH, J. FRERICHS, and R. LICKLIDER, Washington University, St. Louis.

The Elimination of the Blind-Alley Response of a Temporal Maze Problem. SOL EVANS, Ohio State Bureau of Juvenile Research.

Pseudo-Conditioning Without Paired Stimulation Encountered in Attempted Backward Conditioning. W. F. GRETHER, University of Wisconsin.

The Function of Stimuli in Maze Learning by Rats. CHARLES HONZIK, University of California.

Studies in the Learning Function. L. E. WILEY and A. WILEY, Ohio Wesleyan University.

SESSION C. SENSORY PHENOMENA I

CHAIRMAN, S. RENSHAW, OHIO STATE UNIVERSITY

The Bright Visibility Curve of the Rabbit. R. H. BROWN, Yankton College.

Difference Thresholds for Sour Taste of HCl and H_2SO_4 as a Function of pH. R. B. MALMO, University of Missouri.

Sensitivity to Cold and Vascular Constriction in Asphyxia Block. J. P. NAFE, Washington University.

Studies in Monocular Depth Perception. M. C. VAN TUYL, University of Michigan.

Further Evidence of Color Discrimination in Rodents. W. E. WALTON and R. W. BORNEMEIER, University of Nebraska.

SATURDAY AFTERNOON, MAY 8

SESSION A. AFFECTIVE PHENOMENA

CHAIRMAN, W. L. VALENTINE, OHIO STATE UNIVERSITY

Variability in Preferences for Simple Polygonal Forms. GILBERT BRIGHOUSE, University of Illinois.

The Influence of Muscular Tension and Relaxation on the Affectivities. HULSEY CASON, University of Wisconsin.

Preference for Auditory Rhythm. WILLARD HARRELL, University of Illinois.

The Aesthetic Experience: A Psychological Description. KATE HEVNER, Bloomington, Indiana.

A Comparison Between Electrodermal Response and Hedonic Tone. C. E. JURGENSEN, University of Iowa.

Factor Analysis in the Study of Emotional Response by Methods of Impression. RICHARD LEDGERWOOD, Southeastern Teachers College.

How Accurately Can Music Express Meaning? MELVIN RIGG, Kenyon College.

SESSION B. CONDITIONING

CHAIRMAN, H. F. HARLOW, UNIVERSITY OF WISCONSIN

Subliminal Conditioning of Pupillary Responses. L. E. BAKER, University of Wisconsin.

Reaction Time Studies Relative to Skill in Certain Sports. DOROTHY BEISE and VIRGINIA PEASELEY, University of Michigan.

Conditioned Vasomotor Responses in Human Subjects. RODERICK MENZIES, University of Wisconsin.

Higher-Order Conditioning and Verbal Learning. H. N. PETERS, University of Missouri.

The Effect of Electric Shock on Learning in Eye-Hand Coördination. R. C. TRAVIS, Western Reserve University.

SESSION C. SENSORY PHENOMENA II

CHAIRMAN, M. A. TINKER, UNIVERSITY OF MINNESOTA

Vibro-Tactile Stimulation an Auxiliary to the Lip-Reader. R. H. GAULT, Northwestern University.

The Sensibility of the Dog's Foot to Vibro-Tactile Stimuli. L. D. GOODFELLOW, Northwestern University School of Medicine.

The Effect of Binaural Occlusion of the External Auditory Meati on the Sensitivity of the Normal Ear for Bone Conducted Sound. N. H. KELLEY, University of Iowa.

The Influence of Phase on Timbre. DON LEWIS and M. J. LARSEN, University of Iowa.

The Relation of Time Estimation to Certain Physiological Changes. VERNON SCHAEFER, Northwestern University.

REPORTS FROM THE LABORATORIES

CHAIRMAN, J. C. PETERSON, KANSAS STATE COLLEGE

SATURDAY EVENING, MAY 8

ANNUAL DINNER

Toastmaster: C. A. RUCKMICK, University of Iowa.

Address of Welcome: DEAN H. F. FLETCHER, University of Illinois.

Address of the President: H. A. CARR, University of Chicago.
Subject: *The Search for Certainty.*

PROCEEDINGS OF THE ROCKY MOUNTAIN BRANCH OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

T. R. GARTH, SECRETARY, UNIVERSITY OF DENVER

SUMMER MEETING

The summer meeting of the Rocky Mountain Branch of the American Psychological Association was held at the University of Wyoming, Laramie, Wyoming, July 25, 1936. The following program was presented:

MORNING SESSION

CHAIRMAN, ROBERT H. BRUCE, UNIVERSITY OF WYOMING

Differences in Output Under Various Work Conditions. S. L. CRAWLEY, Western Reserve University.

Some Experiments on Bilateral Transfer in Preschool Children. ADA HART ARLITT, University of Cincinnati.

Some New Experimental Results on the Speed of Perception of, and on the Memory for, Visual Forms. SAMUEL RENSHAW, Ohio State University.

A Preliminary Study of Hands and Faces. T. R. GARTH, University of Denver.

AFTERNOON SESSION

CHAIRMAN, GEORGE T. AVERY, COLORADO STATE COLLEGE

Papers and Informal Reports Centered Upon the Problems of Motivation and Learning

An Experimental Study of Habit Regression in the White Rat. LARRY O'KELLY, Ohio State University.

Some Recent Experimental Work Upon the Relationship of Motivation to Learning. KARL MUENZINGER, University of Colorado.

A Preliminary Report Concerning the Effect of Drugs Upon Behavior. JOHN SEWARD and GEORGENE SEWARD, Columbia University.

A Preliminary Report of a New Technique to Investigate the Effect of Social Factors Upon the Thirst Drive in the White Rat. ROBERT H. BRUCE, University of Wyoming.

EVENING SESSION

CHAIRMAN, THOMAS HOWELLS, UNIVERSITY OF COLORADO

Factors Effecting Family Size. JOHN E. ANDERSON, University of Minnesota.

FALL MEETING

The fall meeting of the Rocky Mountain Branch of the American Psychological Association was held at the University of Denver, Denver, Colorado, November 27 and 28, 1936. The following program was presented:

FRIDAY, NOVEMBER 27

MORNING SESSION

A Study of Behavior Patterns of the Ring Dove to Multiple-Choice Problems. GEORGE T. AVERY, Colorado State College.

Some Results Obtained from the Uses of Fernald's "Kinaesthetic Techniques" in Learning to Read and Spell. W. T. WAIT, Colorado State College of Education.

The Relation of the Median Standardized Achievement Test Scores of College Entrants and the Number of Different Kinds of Literature Which They Like. J. D. HEILMAN, Colorado State College of Education.

A Report of Progress in the Study of the Foster Indian Child. T. R. GARTH, University of Denver.

Report on Business Barometer Studies. RICHARD J. TRIPLETT, University of Denver.

Recognition Versus Recall Methods of Testing Spelling. EVELYN M. STURDYVIN, University of Colorado.

A Personality Test for Children. EDNA D. BAXTER, University of Denver.

AFTERNOON SESSION

Joint Meeting with the Educational Section of the Colorado-Wyoming Academy of Science

A Re-Formulation of the Law of Effect. K. F. MUENZINGER, University of Colorado. Discussion, led by R. A. DAVIS, University of Colorado.

Vocational Training and Guidance in Education. C. G. HOUSTON, Grand Junction State Junior College. Discussion, led by G. S. WILLEY, University of Denver.

SATURDAY, NOVEMBER 28

A Description of Reaction Time Apparatus. EDGAR HOFFMAN, University of Denver.

Factors Influencing Social Adjustment in Children of Preschool Age. LILLIAN G. PORTENIER, University of Wyoming.

Some Phobias and Systolic Blood Pressure. L. W. COLE, University of Colorado.

A Study of the Goal Gradient with White Rats as Subjects. ARTHUR BERNSTONE, University of Colorado.

A Study of the Goal Gradient with Human Subjects. DOROTHY MARTIN, University of Colorado.

The Rôle of Vitamin B in the Development of Intelligence. EMILY POE, University of Colorado.

A Preliminary Report on a Study of Recreational Therapy. GRANVILLE B. JOHNSON, University of Denver.

Color Preferences of Turks. JEANNE FOOTS, University of Denver.

MINUTES OF THE BUSINESS MEETING

The Rocky Mountain Branch of the American Psychological Association met in a business session on Saturday, November 28, 1936, at 9:00 A.M. Since the president, Dr. Thomas H. Howells, and the vice-president, Dr. Robert Bruce, were absent, Dr. Thomas R. Garth, the secretary, presided.

It was moved that a secretary's book be purchased, and a fund of $2.70 was contributed by the members present. This was placed in the hands of the secretary for purchasing a suitable permanent record book for the minutes of the Branch.

Dr. George T. Avery reported on the status of the Branch with reference to affiliation with the A.P.A. to the effect that its affiliation is secure.

Reference was made to the summer meeting in Denver of the A.A.A.S., and it was suggested that the summer meeting of the Rocky Mountain Branch be made coexistent with the meeting of Section I of that body. Action on this was deferred.

Richard Page made a report on the legal status of the consulting psychologist in Colorado. It was decided by the body to look into the matter of making the status of consulting psychologists legally secure. A committee was appointed by the chairman to do what it could to bring this about, dependent upon consultation with the officers of the Branch. The committee was named by the chairman and is to be composed of the following: Dr. George T. Avery, chairman; Dr. L. W. Miller and Richard Page.

The following officers for the ensuing year were elected: Thomas R. Garth, president; W. T. Wait, vice-president; and K. F. Muenzinger, secretary.

SPECIAL REVIEW

LEWIN'S PRINCIPLES OF TOPOLOGICAL PSYCHOLOGY[1]

BY EDNA HEIDBREDER
Wellesley College

It may be assumed that anyone who is seriously interested in psychology as a science is familiar with the salient features of Lewin's thought. In his separate papers on special topics and in the collected papers called *A Dynamic Theory of Personality*, he has introduced his concepts to the psychological public and has shown them at work in particular situations. In the *Principles of Topological Psychology* he undertakes a different task. Here he sets forth the foundational notions of his system with the primary purpose of exhibiting the conceptual structure of the science as a whole—of revealing the logical framework within which he proposes to deal with all psychological material and with all the relevant material from other sciences that constitutes its setting. To be sure, the book does not give a complete account of Lewin's system. Vector psychology is not included. But it is abundantly clear from topological psychology alone that the book attempts a revision of psychological thinking which affects radically not only the practices of experimental investigation, but the very manner in which the subject matter is conceived.

And yet in a sense, Lewin does not present a system at all; certainly his psychology is not a system as that term is commonly understood. What he does present is a conceptual tool, but a conceptual tool which promises to give psychology the unity, coherence, and articulation that systems regularly try to achieve, and which, like most systems, implies a characteristic method of research.

In the letter to Köhler that constitutes the preface, Lewin writes:

> "I remember the moment when—more than ten years ago—it occurred to me that the figures on the blackboard which were to illustrate some problems for a group in psychology might not after all be not merely illustrations but representations of real concepts. . . . I began studying topology and making use of its concepts, which soon appeared to me particularly fitted to the specific problems of psychology." (P. vii.)

[1] Lewin, Kurt, *Principles of Topological Psychology*. (Trans. by Fritz and Grace Heider.) New York: McGraw-Hill Book Company, 1936. Pp. xv+231.

These sentences express in the briefest possible form the core of Lewin's conception. His proposal is to use mathematical concepts which are spatial rather than quantitative. As mathematical, they possess the logical rigor that is characteristic of mathematics generally, and as spatial, Lewin believes that they possess a special suitability to the empirical subject matter of psychology, particularly in its dynamic aspects. By means of these tools a system will be achieved which is "oriented in two directions, namely, toward theoretical connectedness and toward concreteness . . . equally suitable for the representation of general laws and of the characteristics of the individual case" (p. 5). Anyone acquainted with Lewin's previous writings will recognize this kind of system as a construct characteristic of the Galilean as opposed to the Aristotelian mode of thought. By its adoption Lewin believes that psychology will become a deductive science, possessing the logical rigor of mathematics as well as complete and immediate relevance to concrete individual cases—something as different from the empirical psychologies of today as contemporary physics is from the old science of natural philosophy.

It is important to recognize explicitly and immediately that the space Lewin refers to is not perceptual, three-dimensional, Euclidean space. Lewin especially emphasizes the point that he is not dealing with physical space, the only space customarily considered in a scientific as contrasted with a metaphysical way. He points out that mathematics has developed a concept of space which is by no means limited to physical space or to any space that can be visualized. As a matter of fact, mathematical space can be developed from different fundamental relationships. One of these—it is not hard to see why a Gestaltist finds it especially significant—is the whole-part relationship. By means of certain series of inclusion, it is possible to characterize the concept of a point and also the concept of a surrounding. Such a procedure gives topological space—a space in which mathematical relationships are dealt with without measurement; in which a drop of water and the earth are fully equivalent, as are also a sphere and a cube; in which only certain relationships and the possibilities of certain relationships are relevant. Lewin further calls attention to the fact that a highly developed branch of mathematics has grown up about the concept of connectedness. It deals with such matters as separated and connected spaces, different kinds of connectedness, regions, boundaries, and cuts. Even problems of dimension can be treated on the basis of topological concepts without recourse to measurement.

A single and rather simple illustration of the way in which topological concepts may be used in psychology is given in the following passage. Here the author attempts to show how psychological changes which are dynamically important may be correlated with such topological concepts as locomotions, paths, boundaries, and regions.

> "The vocational goal of a sixteen year old boy is to become a physician. The 'path' to his goal leads through definite stages: college-entrance examinations, college, medical school, internship, establishing a practice. . . . When he passes his college entrance examinations he has made 'a step forward' on the way to his goal. This movement is certainly not a bodily one. Nevertheless it is a real locomotion, a real change of position. . . . The reality of the change in his position becomes clear when one considers that many things are now within his reach which were not before. He can go to college or university, his time is much more within control than before. His social position, too, is changed: he can play on the college football team, go to dances, etc. His examinations therefore had for him the character of a boundary between two distinct regions. He had to cross this boundary if he wished to go from the one region to the other." (P. 48.)

In a similar manner, using the conception of space just outlined, it is possible to represent any mental event as a function of a given situation, the situation including both the person and the environment. The basic formula is $B=f(S)$, with B standing for behavior and S for situation; or more fully, $B=f(P\,E)$, with P standing for person and E for environment. The total situation is the life space, the totality of facts that determine the behavior of an individual at a given moment.

The life space can be represented completely and unambiguously in terms of topological concepts. This representation, it need hardly be said, is not limited to physical or geographical space from the standpoint of the physical sciences. It includes also quasi-physical, quasi-social, and quasi-conceptual facts—the facts of the physical world, of actual social situations, and of actual conceptual relations, not as they are determined objectively by the relevant sciences, but only to the extent and in the manner that they affect the individual at the moment considered, regardless of whether or not the way in which they figure in the life space corresponds to the objectively determined facts. Furthermore the life space includes *only* those facts of the objective world (physical, social, conceptual) which are relevant to the situation considered; it is not necessary, for example, to take into account the total state of the physical universe in the illustration just quoted. It is important, too, to recognize that the life space includes the person as well as the environment, that the

person like the environment is structured, and that the parts and connections within the person can be represented by topological concepts.

Finally, it must be distinctly understood that this treatment of the life space takes into account the special nature of the particular situation; otherwise a full understanding of the PE in the formula would be impossible. In Lewin's thinking the single case is immensely important. The f in the formula represents a psychological law, and psychological laws define functional relationships between different characteristics of an event or situation. But knowledge of psychological laws is not enough. "Even if all the laws of psychology were known, one could make a prediction about the behavior of a man only if in addition to the laws the special nature of the particular situation were known" (p. 11). No concept can be adequate to the needs of psychology which does not take into account both general law and individual case, thus giving psychology both the 'theoretical connectedness' and 'concreteness' previously mentioned. That the concepts of topology do precisely this is a theme developed throughout the book. The concept of the life space, the central concept of topological psychology, is the example *par excellence* of this mode of thinking. It is a construct in which the opposition between particular case and universal law is overcome.

No one who is acquainted with the compactness of Lewin's style will expect a summary of his system. His exposition moves forward not at all rapidly, but with great economy of expression. Only the barest outline of the book can be given. Part I is entitled "The Task of Psychology and the Foundations of Topological and Vector Psychology." It is a critical exposition of the theoretical basis of his entire system. Part II is entitled "Topological Psychology." It presents the concepts which are fundamental to the representation of psychological situations, and actual representations in terms of those concepts of the environment and of the person. It also includes a chapter on the difficult problem of dimensions of the life space.

It is possible, however, though the book resists summarization, to convey much of the tone of the author's thinking and even to suggest something of the character of the system itself by considering some of the objections that have been raised against it, together with the ways in which Lewin has met them. It is also interesting to note in this connection the ways in which some of Lewin's concepts differ from those of closely related systems.

One of the most immediate and frequent objections is that the representations that Lewin thinks of as topological constructs are

mere illustrations of psychological situations, and that as such they merely present in another way what is already known. To Lewin this objection rests on a simple and fundamental misconception of his intention. The drawings or diagrams he gives as representations of a psychological situation are not in any sense given as visual pictures of it. Neither are they intended as mere expository devices for bringing out the abstract relationships with which they deal. On the contrary they are a graphic means of representing the logical structure of the relationships themselves, and as conceptual representations, they are to be taken no less strictly than is mathematical representation in physics. Anyone misses the point entirely who regards the diagrams and figures with which the conceptions are represented as more or less than means of indicating the topological structures which represent psychological facts conceptually. The diagrams and figures themselves are of secondary importance, and are even misleading if interpreted in terms of the usual metrical geometry. They represent the situation not perceptually but conceptually. They neither are nor pretend to be pictorially faithful to the facts; they represent mathematical concepts, and it is the special characteristic of mathematical concepts as distinguished from other symbols, such as those of ordinary speech, that they belong to a system of concepts which are related to each other in a univocal way. "The scope and unambiguousness of these relationships are what makes the coördination of mathematical systems to real facts so fruitful for investigation and this is no less true for psychology than for other sciences" (p. 78). It is true that they add nothing to what is already known in the sense that they disclose no cause *behind* the facts presented; but the topological conception implies a notion of cause and explanation quite at variance with the one suggested. It is one, however, which can best be discussed after further consideration of Lewin's attitude toward a topic closely related to that of illustration.

This topic is the use of models in psychology. In general, the same arguments apply to models as to illustrations, and it is in this connection that Lewin explicitly states that his avoidance of the use of models, physical and non-physical, is intentional. A model, like any illustration, always contains something that is purely arbitrary. "One uses it," he says, "like an illustration only insofar as the analogy holds, *i.e.* really only as long as it is convenient" (p. 79). On the other hand, if a real fact is represented by a mathematical concept, all the consequences involved in the concept must be acknowledged, and a science "will obtain the real benefit of the application of mathematical concepts only if it uses them in an absolutely binding way" (p. 79).

In particular, Lewin makes it clear that he has no intention of using physiological models as means of *explanation* in psychology, and his whole conception of causality is bound up with this fact. No one needs be told that there is a marked tendency in psychology to establish a close kinship with physiology. Insofar as this practice consists in making use of physiological methods and in including physiological facts in psychological theories, Lewin is altogether in favor of it. But whenever the physiological theories give rise to physiological (or physical) models which are offered as explanations, he is forced to protest. His objection is implied in his objection to models in general: the relationships are expressed not directly in concepts, but indirectly through illustrations.

But it is the task of psychology, as Lewin conceives it, to determine and represent directly the dynamic characteristics of the psychological facts themselves, and he reminds the reader that there are psychological techniques and methods of experimentation through which these determinations and representations can be made directly. Psychology is concerned with determining more and more exactly the properties of psychological systems themselves—psychological systems in their full, concrete, psychobiological reality. And since the properties of these systems as known by psychological techniques are already the properties of the real psychobiological systems, to coördinate physiological systems to them is a meaningless duplication.

It is distinctly clarifying, in considering this point, to note Lewin's comments on Köhler's concept of isomorphism. That concept, he believes, whatever its value in other respects, contributes nothing to an understanding of the relationships between physiological and psychological systems, for the simple reason that in dynamic psychology the scientist is dealing not with a duality but with identical systems. As a matter of fact the methods Köhler uses to determine the structures and properties of the brain field are essentially the same as those Lewin uses to determine the structure and properties of the life space. Any explanation in terms of dynamic relations goes back to assumptions about the structures and the properties of the basic facts that seem to be equivalent in their mathematical form, regardless of the terminology—brain field or life space—in which they are expressed. But it is the relations themselves that are important and Lewin believes that they should be investigated and represented directly. To use a physiological model—brain field or any other—only creates the illusion of a transition to a greater degree of reality without actually advancing the conceptual determination

of the dynamical properties at all; an illusion presumably occasioned by connotations of the old mind-body distinction with its implication that the bodily is somehow more real and objective than the mental.

It might be supposed that, since brain-field and life-space are so similar in the relations they represent, it would be a mere matter of convenience whether those relations were expressed in terms of physiological structures or of mathematical concepts. But the difference is an important one in Lewin's thought. The duplication that isomorphism involves is not merely unnecessary; it may be positively harmful. By dragging along an outmoded way of thinking, it may impede the progress of the true function of the application of topology to psychology, that of making psychology a logically rigorous deductive science by means of 'constructive' concepts which engage directly with its special material.

It is interesting, too, that Lewin also disagrees with Koffka's distinction between the behavioral and geographical environment. He finds Koffka's conception unsatisfactory because it assumes that immediate experience is directly given and physical facts merely inferred, and also because it further implies that all explanation—all 'conditional-genetic factors,' in Lewin's terminology—must be found in the physical world. Lewin formulates his position on this problem in one of the most telling and most characteristic passages in the book.

> "The objects of all empirical sciences, including the objects of physics, can be experienced no less directly than those of psychology. This direct experience concerns first of all the appearance of objects—*i.e.*, their phenomenal properties. In order to understand causal relationships, one has to proceed to the conditional-genetic properties. But this progression to deeper levels takes place *within one and the same field of science.* The conditional-genetic properties of a piece of iron that physics finds remain the properties *of this same piece of iron which one perceives and uses,* however far the concepts of physics may progress and however indirect physical methods may be. Otherwise the scientific analysis would be meaningless both from a practical and from a theoretical point of view." (Pp. 20–21; italics are the reviewer's.)

The continuity of concept and percept in science, and the concreteness of the reality with which science deals, could hardly be expressed more clearly.

It is this procedure—constructive thinking in this sense—that topological psychology attempts to follow. The starting point is the phenomenal fact—its appearance. But in order to understand it, one must progress to conditional-genetic properties, using concepts, as physics does, which may travel far from the phenomenon itself and which may involve very indirect methods, but which nevertheless

retain a continuity with the phenomenon and which take place within one and the same field of science. If psychology uses this method, it will not have to leap into another science—physiology or any other—whenever it needs an explanation. It is in this sense that topological psychology prefers concepts that deal directly with concrete reality.

Lewin, then, disagrees with both Köhler and Koffka on the question of the relation of the subject matter of psychology to that of physics and that of physiology. What is his own interpretation of the relation? First of all, he believes that the problem must be attacked from the standpoint of the theory of science—that instead of contrasting the physical and the mental, or the body and the mind, one must consider the differences between physics and psychology, or physiology and psychology. Considering the problem from this standpoint, he regards the facts of physics as belonging to one class, and those of psychology and physiology as belonging together in another class. He particularly emphasizes the point that the bodily, physiological facts and the psychological facts are together; and that together they belong to the class of biological facts which is contrasted with the class of physical facts. He hopes in this way to make it clear that in speaking of psychological regions, forces, or changes, he is dealing with facts that have the same kind of reality and the same kind of existence as biological facts in general, and to assure the reader that he is not dealing with mere 'figments of the imagination.'

What, then, is the difference Lewin finds between physical facts on the one hand and biological (including physiological and psychological) on the other? It is worth considering this question in some detail because Lewin's answer shows topological concepts actually at work in a characteristic way on a problem that is unquestionably fundamental. His aim is to make a distinction that does not depend on the properties of the physical and the mental as those terms are ordinarily used, but that indicates instead in terms of mathematical concepts, how the science of physics differs from the biological sciences such as physiology and psychology. His general position is that the difference lies in the way in which the temporally distinguished units of the existential series (*Genesereihen*) are related to each other in the contrasted fields. This difference in the temporal order determines a difference in the spatial order.

More specifically, space in physics is conceived as one single connected space that includes all existing physical realities. This means that the physical world is dynamically a complete and closed system.

In accounting for the way any given situation came into being, the regular procedure in physics is to trace it back to an earlier situation. In other words, if one begins with a temporally later situation, S_2, one traces it back to a temporally earlier situation, S_1, and shows that S_2 is completely accounted for through the events that resulted from S_1. Occasionally, it is true, one discovers, in following this procedure, that S_2 cannot actually be derived from the S_1 chosen. But this simply means that the particular S_1 was incorrectly chosen—that it was not sufficiently comprehensive and that therefore certain facts were neglected which in the interval influenced the system from outside. But the significant point is that in physics, such errors can always be corrected. In physics it is always theoretically possible to choose an S_1 so comprehensive that S_2 can be derived from it. The facts of S_1 always exist physically and always have a place in physical space.

But this procedure is not always possible in biology, and hence in psychology. Lewin illustrates this point by means of a simple and commonplace psychological situation. While a person is writing a letter, a door may open and another person may enter the room unexpectedly. The behavior (B_1) of the letter writer up to the opening of the door may be derived completely and satisfactorily by psychological laws from the situation (S_1) before the door opens. Similarly, when the door opens and the visitor enters, the behavior (B_2) of the person in the altered situation (S_2) may be derived completely and satisfactorily by psychological laws from that situation (S_2). But the important point is that S_2, as a psychological situation, cannot be derived from S_1, as a psychological situation. If, following the procedure of physics, one tries to make S_1 more inclusive so as to include the approach of the visitor, he runs counter to the plain psychological fact that the writer did not actually know of the visitor's approach and that the visitor and his behavior were in no way a part of the psychological situation. To put the matter somewhat differently, there are changes in the life space which cannot be derived from the dynamics of the life space itself, even if one assumes a strict determinism and even if one has a complete knowledge of the given psychological situation and of all psychological laws. It must be recognized that the psychological life space is influenced by factors alien to psychology—by factors both numerous and various. In contrast with the single dynamically closed system of physics, psychology deals with a plurality of systems which are dynamically unclosed.

But not for a moment must it be supposed that psychology is

therefore compelled to abandon a strict determinism. It is entirely possible to represent mathematically a system that is dynamically unclosed, and yet subject to strict determinism. Such systems are of two sorts: the influence from without may affect only certain regions of the life space, or they may affect it at every point. In the former case, which is the simpler of the two, there are inner points and boundary points. In this case the life space would be represented by a limited and closed region—a region that includes its own boundary. Only psychological laws would apply to the inner points and alien influences would affect the boundary points. Even in the apparently more difficult case in which outside influences may affect the life space at every point, mathematical representation is possible. Here all the points are boundary points, and the situation can be represented by embedding the space of psychological facts in a space that has one dimension more—a procedure always possible in topological space, regardless of the number of dimensions required for the psychological life space itself.

Lewin believes therefore that the concepts of topology are entirely adequate for representing the relations within the psychological life space, and what is more, between the psychological life space and the physical world. Furthermore, they have the special advantage of making unnecessary any assumptions as to a mechanical relation between the mental and the physical; they can represent the observed relations directly. There is no cluttering up of the problem with metaphysical considerations or considerations from common sense. Again it is apparent that topology is capable of providing constructs that engage directly with the subject matter under consideration.

From this point of view it is easy for Lewin to dispose of the criticism, suggested earlier, that his representations do not really explain. This criticism may be formulated more fully by saying that it is self-evident that one can derive from the representation all the events represented; since the representations are after all *mere* representations, they contain only what is already known of the situation; they merely describe; they explain nothing at all.

Lewin's answer is that it is perfectly true that the representations do not explain if by explanation is meant the discovery of something *behind* the events from which they are derived. To one who holds fast to this notion of causality, all explanations in terms of constructive or Galilean thought must be disappointing. But the whole point of Lewin's argument is that it is impossible for science to retain this conception of causality. Scientific explanation does not consist in

disclosing behind an event a cause belonging to a different order of some sort, but simply in progressing from the phenomenal to the conditional-genetic in the same order of reality. This is, in fact, the only kind of explanation in line with the development of science in general—an account in terms of the dynamic relationships themselves, not in terms of entities of any sort which are discovered behind the facts. If something more is expected, if such explanation seems somehow to evade the real issue, the reason lies, Lewin apparently believes, in a not yet perfect adjustment to the Galilean way of thinking in psychology, an adjustment similar to that which had to be made to the concept of explanation in physics at the time of Galileo and Newton.

Out of comparisons such as these, and out of objections and answers to objections, the fundamental concepts of Lewin's system emerge. They include topological space which is not to be confused with physical space or limited to Euclidean space of any sort, mathematical concepts which are absolutely and univocally binding and which, when presented visually and diagramatically, are under no conditions to be considered mere illustrations; a method of explanation which makes the use of models of any sort unnecessary and possibly misleading, and which employs constructs of thought that directly represent the observed facts and relationships themselves; relationships among the sciences which are to be understood not in terms of any metaphysical entities like mind or matter, but in terms of the theory of science; and a goal of scientific endeavor which is understood to be the complete representation of a single case which takes into account all the particular characteristics of the individual case and at the same time represents laws which are absolutely general. The main contention is that the conceptual tools developed in one science, topology, will make possible the solution of the fundamental and pressing problems of another science, psychology, and will thereby bring about the unification of that science and clarify its relationship to other natural sciences.

Any system that attempts such a task is sure to encounter two obvious questions: (1) Is the use of the tool logically permissible? (2) Is it practically profitable—*i.e.* does it actually accomplish anything?

On the first of these questions Lewin is very explicit. His general position is that from the standpoint of mathematics, there can be no objection to the application of mathematical concepts to psychological facts, because the "nature of the things whose system consti-

tutes a mathematical space is entirely irrelevant for modern mathematics. It does not matter whether one thinks of them as physical objects, temperatures, numbers, colors, events, or anything else" (p. 52). The question, then, is one of the suitability of topological concepts and the subject matter of psychology to each other. Can topology supply the appropriate concepts? And is psychological material amenable to this kind of mathematical treatment?

Lewin has already called the reader's attention to the rôle of the whole-part relationship and the concept of connectedness in the development of topological space. "It is now generally recognized," he says, "that the whole-part relationship and the relationship of parts to each other play a fundamental rôle in psychology" (p. 54). On this assertion—which, it should be noted, is offered without discussion simply as a statement of generally recognized fact—he bases his case that topological tools are suited to psychological material. He recognizes and discusses certain difficulties from the mathematical side—particularly the problems of distance and direction which go beyond topological determinations—but he does not regard any of them as insuperable.

From the psychological side, too, he recognizes difficulties. Lewin admits that in general the application of spatial concepts is far more difficult in psychology than in physics, for psychology from the very beginning has to base its spatial concepts on dynamics whereas in physics only the most recent developments, especially those centering about the concept of relativity, have confronted the science with that problem. The instability of psychological situations has also been urged as a difficulty, but even if all psychological situations displayed this characteristic—which as a matter of fact they do not—it would constitute no obstacle to topological treatment, since in the application of mathematics it is altogether irrelevant whether a given situation represents a section through an event that occurs with great rapidity or through a relatively constant situation. A more serious problem is raised by the objection that the indeterminacy of psychological events may be considered an obstacle to psychological treatment. For example, the child's perceptual field may be so undeveloped and so unstructured as not to permit the application of even the simplest concepts of parts or connectedness. The answer to this objection is, in effect, a declaration of faith:

> "It seems to me that one of the most important general characteristics of the psychological life space is that it is not infinitely structured, but that it is always structured only to a certain degree. . . . For the present discussion of

topological questions it suffices to say that the life space is to a certain extent structured. At least there is a certain topological structuring of the environment in nearly all situations with which psychology deals, and no doubt there is always some structuring of the person. This supplies the empirical premise for the application of the topological concepts to the life space." (Pp. 61–62.)

In other words, the answer is that in the author's opinion—to the best of his knowledge and belief—the nature of psychological material is such as to permit the treatment he proposes. Obviously his case stands only if his 'empirical premise' stands.

The second main question is one that cannot possibly be answered by the system itself. The practical value of the principles can be determined only by the outcome of laboratory research, a fact of which Lewin is entirely aware. The point is mentioned only because it must not be supposed that the second part of the book—the chapters that work out in detail the topological representation of particular psychological situations—is in any sense an answer to that question. In these chapters the outline of topological psychology itself is presented, as contrasted with the theoretical basis of topological psychology which is given in the first part of the book. It is the great merit of these chapters that they do not stop with the general principle that topology *can* be useful, but take the next step of showing just *how* topological concepts may be applied to actual psychological materials. Their bearing, however, is on the first question, that of the suitability to each other of the conceptual tools of topology and the empirical subject matter of psychology. The question of the practical value of the system for research—aside from the fact that it is a possible, stimulating, and suggestive conception—must be answered in the words which have become the almost inevitable *cliché* on such occasions: it depends upon the experimental evidence.

But beneath both these questions, which can be answered in somewhat general, almost stereotyped terms, is the really vital question of the whole mode of thought by which Lewin proposes to attack the problems of psychology afresh. This is, in his own terminology, the Galilean as opposed to the Aristotelian method—or rather, a special form of the Galilean method constituted by the use of topological concepts in psychology. It is quite outside the scope of this paper to attempt an evaluation of the distinction between Aristotelian and Galilean thought. It is sufficient to note that Lewin identifies the Galilean method with the 'constructive' thought of modern science—'constructive' as opposed to the 'speculative' method which characterizes Aristotelian thought, and to the 'descriptive' method which

he regards as a transition between the two. It is the Galilean or 'constructive' method that he definitely proposes to follow, and in order to understand his intention it is important to consider not so much the value of the distinction, but its special implications for psychology, and to consider it from the standpoint both of scientific methodology and of the psychology of the thought processes involved.

For the sake of clarity, it should be understood at once that the question is not primarily a historical one, as the rather unfortunate names Lewin uses might suggest. The decisive point is not whether there is one set of psychological processes that characterized the actual historical Aristotle and the period of time commonly called the Middle Ages in Western Europe, and another that characterized the actual Galileo and the period of time during which the growth of modern science occurred. To Lewin the question is one of logic, of methodology, and of the theory of science. His concern is with the rôle of scientific concepts in the structure of scientific theory, with their logical and functional relations to each other and to the subject matter with which they deal. His distinction is drawn between certain logical and methodological tools, not between actual cases of thinking considered as historical and psychological processes. But to say that the distinction is made without reference to actual cases of thinking, considered historically and psychologically, is not to say that it refers to static and functionless systems of concepts. On the contrary, the concepts referred to are tools of thought. They are tools for perception on the intellectual plane somewhat as the microscope and telescope are tools for perception at the sensory level. They determine the possibilities and limitations of research in much the same way. But just as the ways in which the microscope and telescope are used is to a large extent independent of the particular biographical incidents, historical and psychological, involved in their invention, and indeed in their development and use by others than their inventors, so the differences between Aristotelian and Galilean thought have no essential reference to the historical periods in which they predominated or to the particular psychological processes of the individuals who use them. In other words, there is a real distinction between concepts as incidents in the life of an individual, and concepts as tools that may be transferred from person to person.

But once this point is plainly recognized, it is enlightening to consider Aristotelian and Galilean thought psychologically as well as methodologically—more specifically, to ask what becomes of the distinction when it is examined from the standpoint of the psychology

of the thought processes. The answer to this question not only sharpens the distinction between thought as methodological tool and thought as psychological occurrence, but also throws into relief what may be a special advantage of topological psychology as a particular form of 'constructive' thought.

Considered as a psychological occurrence, thinking regularly has as its starting point some immediate perceptual situation, and thinking regularly gets away from—goes beyond—the immediate situation in some way. That is, thinking travels from percept to concept, becoming more abstract and general as it goes. (It may, of course, never arrive at a satisfactory concept.) Psychologically, there are many different roads from percept to concept. Different lines of thought take off from points of departure which are psychologically different, and the possible points of departure are as numerous as the ways in which the given situation can be perceived. It is unnecessary to go into the complexities that inevitably occur as the situation develops from the perceptual to the conceptual; but it is important to notice that whatever the psychological procedure, the nature of the psychological processes as such does not determine how the concepts in which they terminate are to be used logically and methodologically. For example, the use to be made of an arithmetical sum is not determined by the particular psychological processes employed in computing it, whether they involved recalling certain number facts, counting on one's fingers, or operating a calculating machine. Just so the concepts arrived at by a given train of psychological activity—and also those developed and used during the activity—may function logically and methodologically in either the Aristotelian or the Galilean manner. This means that the classification of a course of thought as Aristotelian or Galilean tells nothing about the psychological nature of the processes involved. If the attempt is made to apply what is essentially a logical distinction to psychological occurrences—to put actual cases of Aristotelian thinking in one class and actual cases of Galilean thinking in another—the classes, when examined from the standpoint of the psychological processes involved, will be without distinguishing features.

But though the classification of concepts on the basis of logical and methodological considerations is independent of their nature as psychological processes, it does not follow that the usefulness of a concept, logically and methodologically, is entirely unaffected by the psychological processes it involves. On the contrary, the manner in which a concept is used and the field in which it is useful are some-

times determined in a striking way by the special psychological aspect of the immediate perception that serves as the point of departure for conceptual thinking. Mathematics affords an excellent illustration. Mathematics has followed two psychologically different roads from the percept to the concept, one starting from the spatial and one from the quantitative aspect of the immediate perception. Both these lines of thought are capable of high development in the direction of abstractness and generality. They possess many similarities, but throughout their development they retain characteristic differences, and characteristic possibilities and limitations, determined by the particular psychological aspect—spatial or quantitative—of the perceptual situation that formed the psychological basis for their logical development. These two lines of thought are of great importance to Lewin in the development of topological psychology. They are two modes of thinking which he regularly contrasts.

To the reviewer, it is highly significant that Lewin takes the attitude he does toward the quantitative. The opening paragraphs of his introductory chapter are directed straight toward a consideration of the limitations of measurement and statistical treatment in psychology, and throughout the book the author's conviction concerning the inadequacy of 'collections' of facts and 'mere inventories' of behavior is repeatedly expressed. But most significant of all is his insistence, in presenting the 'constructive' or Galilean mode of thought that produced modern science, that the quantitative side of science is only an incidental feature of its characteristic method. He admits that the quantitative 'style' of science forms one of its conspicuous aspects and that scientists themselves are likely to take pride in the fact that their laws can be stated quantitatively. But to Lewin their pride in the quantitative is in a sense misleading because it tends to obscure the point that science is quantitative only secondarily. It is quantitative only because it attempts to be as precise as possible in its primary concern, the representation of the pure case—not a single, particular, historical and geographical case, not even a case which is an average of particular historical and geographical cases, but a 'construct' of Galilean thought. The absolutely rigid measuring rods of physics are a case in point—rods which never have existed and which never do exist and which nevertheless play a recognized part in the science. The laws of modern science are always formulated with reference to such cases—cases which cannot be obtained in a laboratory, and which at best can only be approximated in any empirical and experimental setting, no matter

how strictly the conditions are controlled. Hence the paradox that physics, the most advanced of the natural sciences, the science that has the most unmistakable control of and the most unmistakable reference to the actual empirical world, is by no means derived from averages of actual, empirical cases. The quantitative laws of physics are merely the most precise statements possible concerning pure cases.

But the ways of physics with quantitative tools have not been the ways of psychology. Psychology has concerned itself, Lewin points out, not with pure cases but with unreal averages of actual, empirical, historical and geographical cases. It is possible to state Lewin's objection to these unreal averages by saying that they employ a point of departure from the perceptual situation which makes them, as concepts, inadequate to the problems of a dynamic psychology. In order to quantify in psychology, one must resort to some form of counting or enumeration and therefore to classification and drastic selection. One is thus forced to consider only a particular aspect of the concrete whole, ignoring all other facts about it. In this way one eventually arrives at a value which represents something like the time required by so many children of such and such an age and description to solve a specified problem. By means of this procedure, one obtains data which may be expressed in curves of distribution and in graphs, or in averages and deviations from the average; but whatever the statistical devices employed, the outcome represents the actual activities of no child at all. It represents a fact which may be useful, which may be useful even to dynamic psychology, but which by itself tells nothing of the actual operations that occurred;—that is, nothing of the actual dynamics of the situation. As it stands, it represents an abstract fact, from which everything that can show the actual working of the situation has been discarded. It has lost all concreteness, and all meaningful connection with the concrete things, persons, fields, and situations in which dynamic processes actually occur.

And here, precisely in this defect of the quantitative method, one gets a clue to the positive function of topological concepts. In topological psychology, as in all thought, abstraction occurs. Topological thinking cannot stand rooted in the perceptual situation any more than can any kind of thinking; it cannot save the whole of the perceptual situation any more than can quantitative thinking; it too must abstract from what is perceived. But it abstracts in a way that is different, not logically but psychologically. In selecting as its point of departure the spatial aspect of the concrete situation, it abstracts

no one isolated fact or quality, but one of the very forms of organization that gives the perceptual situation itself unity and wholeness. It thus arrives at a form of thought in which objects and relations can be represented and in which actual operations can be conceived. Not that topological concepts are picturable or imaginable; in this respect they differ from perceived 'things' and 'fields.' But being spatial, they retain, however abstract and general they may become, a characteristic of concrete situations which makes possible the apprehension of a very large number of connections and relationships in a compact, coherent and unified form. In a highly figurative sense, topological concepts may be said to have something like 'thing character'—that is, in abstract thought, they may perform the function of unification with direct relevance to the concrete, which in thought that does not stray far from sensory perception, is performed by what William James called "that powerful *denkmittel* of common sense, the thing." Topological concepts in the realm of the highly abstract and the general, like things in the realm of sensory perception and common sense, may be the means of apprehending in a compact and therefore convenient form, a wealth of relationships. Perhaps 'field character' or 'situation character' would be a better term for referring to this function, but however it is named, it is this character that gives topological concepts the relevance to concrete situations and the appropriateness to dynamic problems, that quantitative concepts lack. Quantitative concepts may represent, among other things, the results of operations, points in the course of their occurrence, certain relationships among them; but spatial concepts may in a unique way represent the operations themselves. It is in this sense that they may be said to possess something of the thing character—or field character or situation character—of perception itself, and to possess it legitimately and without danger of reification; for the very abstractness of non-Euclidean space—its deliberate, conscious difference from perceived space, the fact that it is so clearly an intellectual construct—protects topological concepts more than most concepts can be protected from confusion with existential reality.

It is possible—the point is worth suggesting, though it cannot be developed here—that topological psychology may combine the methodological economy and safety of operational thinking in general, with the psychological convenience and ease of thinking in terms of compact constructs which have something of the character of things. Because they are many abstractions removed from perceived things, they are free both of the limitations and the superfluous

impedimenta of actual perceived objects, disadvantages of 'things' that are retained even in models. At the same time they retain a peculiar relevance to concrete situations which enables them to introduce some of the advantages of thing character into conceptual thought. This statement does not imply that operational thinking dispenses with thing character altogether; but the concepts at which it aims are as clear of that quality as human thought can well be. It does imply that topological psychology, as a constructive system, is an operational discipline,[2] and that topological concepts may have a special relevance to concrete reality which is particularly suited to the needs of psychology. There is a possibility that topological concepts may be peculiarly adapted to represent not only adequately, but compactly and conveniently, the enormously complex network of relationships present in psychological situations; that topological concepts may reveal, in a form which is at the same time condensed and explicit, the logical structure of the empirical relationships that are important to dynamic psychology.

If this possibility is granted to topological psychology, will the topological approach supplant all other methods of attacking psychological problems? The answer lies in the word possibility, for what topological psychology has done so far is to show that its program *may* be a useful one. It has done so, to be sure, both by presenting a refreshingly original and closely articulated conceptual system, and by applying its principles to actual experimental investigations of various problems in dynamic psychology; but as yet its achievements are not such as to justify a claim to unconditional acceptance. Indeed, the reader must be acutely aware that there is a level of criticism that this review has not touched. The sheer originality of Lewin's conception seemed to demand above all else an explication of its positive significance. But there are crucial questions that cannot be ignored. For example, how valid both logically and psychologically is Lewin's derivation of the pure case, both in physics and in psychology? Are the relations between quantitative determinations and the pure case, either in physics or psychology, always as Lewin claims them to be? Do the objections Lewin raises against the quantitative treatment of psychological data apply necessarily to quantitative determination in psychology generally? The answers to these questions cannot even be attempted in a review that is already too long. They are mentioned because of their obvious bearing on the suggestion that a

[2] Lewin defines a construct as "a dynamic fact which is determined indirectly as an 'intervening concept' by way of 'operational definition'" (p. 213).

recognition of the possible advantages of Lewin's psychology does not amount to a claim for its acceptance. Instead of being the only promising conception of psychological material, it takes its place with the other attempts now being made to devise some method, more satisfactory than any hitherto discovered, of conceiving the subject matter of psychology, and especially of conceiving it in such a way as to bring it into relation with the scientific treatment of the rest of the natural world. In doing so, it finds itself engaged in the most important work now going forward in psychology. For contrary to the tradition in which most psychologists of the present day have been trained, the most urgent need of psychology at this particular stage of its development is a really adequate way of conceiving its material—a need which at present is more pressing even than the need for observations and better methods of observation, as great as were the need for observation and methods of observation when psychology was just emerging as a science. If the history of science gives any trustworthy clue, this need will be met, if it is met at all, in close association with observation and experiment, and will be supplied not by a scheme that springs perfect and entire from the brain of any one man, but as the outcome of a widespread coöperative enterprise, in which the participants are moved by anything but an impulse to coöperate. That the need for new methods of conceiving psychology is actively felt is shown by the recent promotion of such varied and vigorously defended theoretical points of view as those stated or implied in the presentation of a molar and purposive behaviorism, in a consideration of the physical dimensions of consciousness, in the many interpretations and criticisms of Gestalt psychology, in the construction of miniature systems of psychology, and in a somewhat different manner, in the psychological concepts that have grown up about factor analysis.

Finally, then, what does topological psychology have to offer? Primarily a conceptual tool which claims special advantages for dealing with the subject matter of psychology. As a mathematical tool, it possesses the logical rigor which characterizes mathematics in general and which promises to make psychology a deductive science in the sense that physics is a deductive science. As a spatial concept, it has a relevance to concrete reality which makes it especially suitable to the problems of dynamic psychology. As a construct, definitely recognized as a construct and nothing else, it has the sophistication of critical scientific thought; and as a construct developed from the spatial aspect of perception, it has something which on the

level of abstract thought is akin to thing character on the perceptual level—something which makes it a psychologically convenient means of representing compactly the logical structure of a wealth of empirical relationships, including those which are important dynamically. Since it is as formal and substance-less as all geometry and since its relevance to existential reality is that of a mathematical tool, it can engage with psychological material and with all material relevant to psychological problems without recourse to models of any sort and without reference to the metaphysical difficulties of the mind-body problem. At present the status of topological psychology is that of a highly original and stimulating hypothesis. Like all scientific hypotheses it will stand or fall—or contribute, along with other hypotheses, toward a more acceptable one—by the usual tests of logic and experiment.

BOOK REVIEWS

TERMAN, LEWIS M., and MERRILL, MAUD, *Measuring Intelligence. A Guide to the Administration of the New Revised Stanford-Binet Tests of Intelligence.* Boston: Houghton Mifflin Company, 1937. Pp. xiv+461.

As indicated by the title, this is essentially a manual for administering and scoring the revised edition of the famous Stanford-Binet tests with only a minimum amount of space devoted to the process of standardization which is to be described in a later publication. Four preliminary chapters, totaling 66 pages, precede the manual proper. Of these, the first three give a general description of the scale, pointing out the reasons why it is believed to be an improvement over the earlier edition, together with a brief account of the process of standardization and a statistical analysis of the results.

In spite of the brevity of this section, or perhaps because of it, the authors have succeeded in giving an exceptionally clear and concise presentation of most, if not all, of the facts essential for the evaluation of the scales. The fourth chapter, entitled "General Instructions" is devoted to the usual and necessary rules for the conduct of an examination. It is clear and well written, but as it contains little that is new no further comment is needed here.

Compared with the old Stanford, the revision has many points both of similarity and difference. Perhaps the most striking resemblance between the two consists in the retention of the age-scale format with its usual correlates of mental age and intelligence quotient. The authors defend this on the ground of its clinical usefulness and the fact that all available statistical procedures for constructing an "equal-unit scale" rest upon the unproven assumption that intelligence is normally distributed. The mental-age scale, they point out, makes no such assumption, nor does it pretend to measure intelligence as linear distance is measured. It tells us merely that the ability of a given subject corresponds (as far as the tasks included in a particular scale are concerned) to the average ability of children of such and such an age. It is a less pretentious instrument, but perhaps comes nearer to fulfilling its claims. After a good deal of experience with scales of the "equal-unit" type, I am inclined to agree with this conclusion.

There are, however, certain difficulties inherent in the mental-age unit which the authors point out and have attempted to overcome. First, there is its decreasing magnitude with advancing age. This is largely self-corrective at the early ages, since the method, by its very nature, requires that the increase in difficulty of the tasks be proportionate to the rate of mental growth. At those ages where growth is most rapid, however, it is apparent that some loss of reliability is bound to occur unless additional steps are provided to make up for the exceptional steepness of the difficulty-curve. For this reason, the authors have grouped the tests for the ages below six years by half-year instead of whole-year intervals; thus providing double the usual number of tasks for each year of age. They have also filled in the gaps that occurred in the old Stanford with tests for the eleven- and thirteen-year levels as well as for the ages of twelve and fourteen years.

Another problem of the mental-age method arises from the fact that mental growth does not cease abruptly but tapers off gradually. The authors have approached this question from an empirical study of the actual scores earned by the older individuals who were tested, and "by a trial-and-error method" arrived at a system of adjustment that meets the pragmatic test of yielding a distribution of intelligence quotients for the older individuals that closely resembles that found for the earlier ages. Briefly, the method consists in a cumulative dropping of one month of chronological age out of every three between the ages of thirteen and sixteen years and disregarding age-increments thereafter. According to this system, a chronological age of sixteen is reckoned as fifteen. This adjustment is made automatically in the table of intelligence quotients at the end of the book and thus is not a source of inconvenience to the examiner.

The new scale has been extended at both ends and the errors of standardization at the extremes that have long been recognized in the earlier edition are believed to have been entirely corrected. The scales now include tests for children of two years and upward, while at the other extreme, three levels of difficulty are provided for the adult. Although it is pointed out that mental ages and intelligence quotients have only artificial significance for older subjects, they have nevertheless been retained as a convenient means of comparison with earlier status. The highest IQ that can be earned by an adult on the present scale is 152.

Two forms, L and M, have been prepared. These are shown to yield mean IQ's that are almost identical at every age, while the

correlations between forms vary from .85 to .95 with a median of .91. For a group of 123 subjects ranging in age from five to twelve years and with an average interval of five months between testings, the correlation between the old Stanford and Form L was .86; with Form M, .85; and with the L-M composite, .88. For 55 subjects between the ages of thirteen and eighteen, the corresponding figures are .88, .91, and .90. Since these values are about as high as the reliabilities of the tests permit, the authors conclude that the new scales are measuring almost exactly the same abilities as were measured by the old.

If this is true, it is perhaps fair to say that the old form might be used as a second alternative scale along with the new. However, it is unquestionably true that the new scales provide a greater variety of items than the old form does and may therefore provide a better basis for clinical understanding. In this connection, however, I cannot refrain from pointing out that a test of general intelligence cannot be made to serve the purpose of a universal diagnostic instrument, and that the practice, so unfortunately common among clinicians, of making pronouncements about special abilities or defects in such broad psychological categories as memory, visual imagery, perception, and the like on the basis of one or two items in a Binet test, is hazardous in the extreme. Needless to say, the authors make no such claim for the new scales, and the point would not be worth mentioning were it not for the fact that a number of people appear to be making it gratuitously.

The work of revision required a ten-year period for its completion and will probably stand for many years as the most thoroughgoing example of test-construction that has ever been carried out. A large number of test-items were selected for a preliminary try-out upon roughly a thousand subjects. A careful evaluation of these items was then made and upon the basis of these results, two provisional scales containing 209 and 199 items respectively were made up. Both scales were given to each of the more than 3,000 subjects upon whom the final standardization was based, half of them taking Form L first and then Form M; while the other half was tested in the reverse order. The final revision includes 129 items in each form with only a few that are identical in the two forms. Exceptional care was taken to insure that a uniform procedure was followed by the seven research assistants who did all the testing, and all responses were recorded in sufficient detail to make later scoring on a different basis possible. The final arrangement, therefore, is the result of an extremely meticulous item-evaluation and should contain a minimum of waste material.

Equal care was exercised in the selection of the subjects for the

establishment of norms. Seventeen different communities in eleven states were sampled, and every effort was made to insure that the group should be as nearly as possible a representative sample of the American-born white child population of the country. Rural as well as city children were used, though the proportion of the former is somewhat less than that reported in the census. Socio-economic status and age-grade location in school were matched with the conditions reported for the general population as accurately as was feasible. Because a comparison of the final results with data available from the census showed a slight excess of cases from the upper occupational levels, an adjustment in the location of test items was made, such as would be expected to yield an average IQ of 100 in a population corresponding to that reported in the 1930 census.

Perhaps the most serious question that can be raised about the new scales is one that has been pointed out by the authors themselves. It is a well-known principle of test construction that the intelligence quotient is a valid interpretative measure only if the standard deviations of mental ages increase in proportion to age. This is equivalent to saying that the standard deviations of intelligence quotients must remain constant from age to age. In neither of the two forms of the revision does this appear to be the case. The trends are very similar for both forms with maximal variance of 20.6 and 20.7 IQ points at the age of two and one-half and a steady decrease thereafter to 12.5 and 13.2 at the age of six. Thereafter the variances again increase, though less regularly, to 20.0 and 19.5 at the age of twelve, with somewhat smaller values for the older ages. The authors discuss a number of possible reasons for these discrepancies which, if true, would constitute a serious problem in the interpretation of test scores, since an IQ on Form L of 162 at the age of two and one-half would have the same meaning as one of 138 at the age of six. Inasmuch as none of the possible explanations can be demonstrated to be true, the authors conclude that "in the lack of positive proof to the contrary, we are probably justified in assuming that the true variability is approximately constant from age to age." It is to be hoped that future findings will show that this assumption is warranted.

An important finding that may explain a number of apparent discrepancies in previous reports on test-reliability is described on pp. 44–47. Here the authors' wording is somewhat unfortunate, though the meaning is apparent. After pointing out that fluctuations in the intelligence quotients of very bright children have been found

to be greater, on the average, than those of dull children they note that this tendency is inherent in the IQ technique and might have been predicted upon logical grounds. "A subject's fluctuation in mental age score will be proportional, not to the variability of his chronological age group but to the variability of his mental age group." Evidently what is meant here is the variability of the chronological age group to which the subject's mental age corresponds. Likewise the last column of Table 11 on p. 46 requires some interpretation. In this table the standard error of an IQ at different levels of brightness as derived from the average difference between the IQ's obtained on the two scales is given and the values so obtained are then transmuted into equivalent reliability coefficients by means of the equation.

$$\sigma_{IQ}=\sigma_{dist}\sqrt{1-r_{11}}$$

The standard errors of prediction vary from 5.24 for IQ's above 130 to 2.21 for those below 70, and the corresponding reliability coefficients from .898 to .982. But the latter values would clearly not hold for the restricted ranges to which they are ascribed; their meaning is purely theoretical and can be expressed only in rather awkward terms prefaced by an "if" that does not hold good. If the standard deviation of IQ were as great for each of the various sub-groups as it is for the entire range (an obvious absurdity) then the reported reliability coefficients might actually be obtained. With the facts as they are, it would seem better to describe the stability of individual standing only in terms of the error of prediction without reference to correlation coefficients that are unsuitable for use with the data.

The guide for giving and scoring the tests which makes up the greater part of the book includes instructions for both forms, a table of intelligence quotients, and a well-prepared index. The instructions are exceptionally clear and sufficiently detailed to make it possible for any experienced examiner who will give sufficient attention to learning the rules to follow the standardized procedure without variation.

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JENKINS, J. G., *Psychology in Business and Industry.* New York: John Wiley and Sons, Inc., 1935. Pp. xii+388.

The ghost of the late E. B. Titchener must be very uneasy of late, to have a book on business and industry emanate from his labora-

tory. Yet there seems little doubt that the traditional precision in experimentation characteristic of that laboratory has left a mark on this book.

The book purports to be an introduction to the field of psychotechnology. It contains sections on industrial selection, industrial production, personnel, industrial distribution, and market research.

I have carried away two main impressions from reading this book. First, the careful thinking, the precautions before accepting conclusions, the exposure of quackeries, and the taking of all variables into consideration, appear in connection with the discussion of every topic in the book.

Nowhere is this better seen than in Jenkins' treatment of distraction, which comprises one part of his excellent chapter on "Conditions of Work." He not only mentions some of the more evident points, such as that blindly soundproofing everything is not only unessential, and that some noises may actually serve to heighten concentration and hence increase output, but also that the nature of the noise and the attitude of the experiencing individual is a more critical matter. Further, not only should output be considered, but the energy demanded to achieve this level of production must be measured. Long time effects, as well as findings from relatively brief experiments, constitute a last item of validation of theories. These lines of reasoning are relayed by the reviewer not so much for their own sake, but as an excellent sample of Jenkins' careful line of reasoning.

My second major impression was that the general tone of the entire book, especially the sections dealing with industrial and personnel applications, was far more destructive than constructive. The traditional subjective methods are blasted; then the weaknesses of tests are exposed. The reader may well ask what is left. Jenkins is optimistic enough about the future of psychotechnology, and is consistently emphatic about the use of strictly scientific procedures, but for the present the reader who has not perused more constructive works might easily become discouraged.

This suggests speculation as to whether such great caution is necessary or desirable. Let us grant that employment tests and efficiency methods are not one hundred per cent effective. There still is a realm between their usefulness and that of traditional rule-of-thumb methods. The more modest and cautious trained psychologists are, the more incentive there is for fakes to be accepted. I have met such a case, where a trained psychologist, after preliminary correlative work, admitted that he could expect to be correct only a certain percentage of the time; the task was turned over to another

with very scanty training but who was not hampered by such modesty.

For these reasons, with which the reader is privileged to disagree, I feel that Jenkins' book is of greatest service to those who already possess some knowledge of general and applied psychology. Possibly the person who makes his first contact with psychotechnology through this text will come away with the impression that psychology has little constructive to offer, and that we make mountains out of molehills. But to those who have already been introduced to the field, this book is exceptionally stimulating and valuable. The author has obviously done a great amount of accurate critical thinking.

The sections on advertising and salesmanship are disappointing, beyond the one major contribution of constant emphasis on the fact that appeal (motivation) cannot be discussed or applied in general, but must be specific to the particular product, the particular customer or reader, and the particular time.

I feel that the best section of the book is that dealing with the principles and techniques of market research. Here we still have the excellent critical analysis, but much more constructive conclusions. These do not necessarily adhere to this field, either, as perhaps there are even more variables in this type of research than in personnel and industrial work. Possibly Jenkins himself has had greater contact with market research, and so has seen that it is effective, even granting that it is not one hundred per cent accurate.

In conclusion, I feel that this book represents a valuable contribution. My opinion is that its proper place is for students who have had some contact with applied psychology, know its uses, and are now prepared for a highly critical evaluation of methods, for this book may be said to deal more with approaches and methods than with facts and principles.

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YOUNG, PAUL THOMAS, *Motivation of Behavior.* New York: John Wiley and Sons, Inc., 1936. Pp. xviii+562.

"At the present time no precise formulation of the field of motivational psychology has been generally accepted by psychologists" (p. 43). This state of affairs is due largely to the fact that it is only within the last decade and a half that the problems of motivation have been recognized at all. American psychology of German origins has long been preoccupied with analyses of sensory and perceptual experi-

ences and their concomitant physiological processes. The British influence has been in biological directions but with main emphasis upon learning and the intelligence *versus* instinct question. And Russian reflexology has been equally neglectful, if not oblivious, of the motivating of the person or organism whose functions, processes, or experiences are being examined. In latter years, however, a few new lines of interest have converged upon the question. One is the psychoanalytic doctrines which, however much they have astonished the psychologist with their *ad lib.* character, have finally aroused him by dint of a constant reiteration of "*cherchez le motif!*" A very different new line of interest is that shift of attention within animal psychology to the identification and measurement of animal drives. Finally, as a third source of—or is it expression of?—the interest in motivation, there is the very recent development of technique for study of attitudes and opinions. But these separately arising young streams have not yet converged; and it is a live problem today as to just how they shall be brought together.

Young's book is an attempt to structure the field of the psychology of motivation. The attempt is quite different from that only other serious one made by Troland in his *Fundamentals of Human Motivation.* Troland reviewed historical and contemporary theories, drew from many sources and added certain redescribed concepts, to mould it all into a systematic conceptualization of the field. Young makes his approach much more obviously empirical, biological, experimental. There is a thread of theoretical organization running through the chapters; but the reader gets somehow a refreshing experience of being taken out on the firing line of contemporary research, so that the very great number of studies described seem to *fall* into a sort of organization rather than to be picked and chosen to *fit* one.

What is the general structuring of the field of motivation offered in this book? "Motivational psychology may be defined as the study of all conditions which arouse and regulate the behavior of organisms. The *arousal* of behavior necessarily implies a release of physical energy from the tissues. The *regulation* of behavior includes the control of activity through purposive determinations, as well as the restriction of activity by organic structure" (p. 45). This may not satisfy us as a definition of motivational psychology—but who has a better one?

After an introduction to the problem (Chapter I) that includes a wide variety of illustrative material—in the concrete, source-book

manner so characteristic of the whole book—the point of approach becomes clearly biological. The animal psychologists' investigations with activity cages, obstruction boxes, and the like, are described with an abundance of graphs and specific citations, under the apt term, 'energetics' (Chapters II–IV). How, out of the biological substratum of animal drive, do there appear purposes as human beings experience them? "To assume a *mental* determinant on the basis of individual conscious experience is just as valid as to assume an inner neural determination on the basis of objectively observed behavior." Yet, "is it necessary to assume anything more than bodily determinants to account for conscious purposes?" (p. 185). When an organism learns a method of satisfying a drive it acquires a goal orientation: it is set or predisposed for a particular kind of final response. The human organism is conscious of many of these predispositions which he attempts to carry into action: they are purposes (pp. 185–186). But what are the *factors* that regulate and direct? Bodily postures, delayed responses, human attentional sets, work sets, quantitative sets, 'will to learn,' habituation, verbal stimuli, attitude measurements, Lewin's tensions, some orally expressed wants of children, and the Freudian wish, all are collected into a chapter (V) that emphasizes the point that "behavior is directed and regulated by bodily postures . . . and by inner neural sets which are the bodily counterpart of conscious purposes and desires" (p. 260).

Directive factors of first importance are reward and punishment (Chapters VI and VII), as shown in many of the typical animal learning experiments. Subjective pleasantness and unpleasantness are shown through a variety of experiments to be "the manifestation within conscious experience of the dynamic interplay of motivating factors" (p. 384); they are symptoms. However, no explanatory theory of the relation is advanced. Social factors in individual motivation are given their chapter (VIII), and results of investigations are arrayed under such heads as prestige, co-workers, rivalry, praise and reproof, and suggestion. And emotion is given another (IX) that presents in contrast many disputed theories and findings, without however attempting a unifying principle—which is just as well!

Finally, the dynamic interplay of competing and of allied motives is so central a theme in clinical psychology that recognition is given to conflicts, adjustments to thwartings, psychoanalytic method, inventories, physiological and other methods (X).

The success of Young's attempt to handle the psychology of moti-

vation in this textbook is to be measured by the reader's aim. If his interest is in system and theoretical integration he will not find it greatly furthered. Not only is the author catholic and eclectic, but his few attempts at theory are not particularly successful. He fails to tie up varying lines of development in convincing manner; and statements of his own positions suffer in clarity. This is true, for instance, in the arguments for his "physical mind" (pp. 526–535).

On the other hand, if the reader wishes to get acquainted with the great variety and great richness of material that is relevant to the topic of motivation, he will find these chapters opening up vistas. And the well-arranged and rather full bibliographies further the opportunity. As a textbook for a discussion class or an undergraduate seminar it should serve well, for the concreteness of the factual material and the whole atmosphere of working in the sources is well calculated to encourage the intelligent student to see psychology in the making. Would that more of our books for advanced undergraduates presented equally solid materials and were equally unpredigested!

As a piece of bookmaking the publishers have done an excellent job with figures, type face, and binding.

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BROWN, J. F., *Psychology and the Social Order: An Introduction to the Dynamic Study of Social Fields.* New York: McGraw-Hill Book Company, 1936. Pp. xiv+529.

If social psychology is to be regarded as a science distinct from psychology and sociology, it must acquire a methodology more complete and satisfactory than any which has yet been developed. Many social scientists and some psychologists have been hoping that the proponents of Gestalt theory would produce such a methodology. Others have wished to see well-known social theories incorporated into a workable social psychology. This book is important because it attempts to accomplish both of these objects. Brown's plan is to interpret the contributions of Marx, Freud, Cooley, Sumner, Spengler, Pareto and others in terms of Lewin's field theory. Even though he may not accomplish this task to the satisfaction of all concerned, he is to be commended for attempting it. He is also to be commended for his frankness in espousing somewhat unpopular views on debatable social issues.

The book is scholarly, well written and contains a minimum of typographical errors. Unlike Lewin, Brown seldom cites factual

references or the results of experiments to prove his points; otherwise the documentation is adequate. The bibliography of 373 titles and the bibliographical notes at the ends of the chapters will be helpful to the general reader, the psychologist, and the social scientist. Summaries at the close of chapters and a résumé of the whole argument are convenient for purposes of review. Appendices on methodology and topology are provided for advanced students. The usefulness of the book as a reference volume is diminished by the lack of an extensive subject index. One searches the index in vain for such items as *barrier, boundary, construct, fluidity, locomotion* and *permeability,* all of which are important technical terms.

The subtitle, *An Introduction to the Dynamic Study of Social Fields,* is perhaps a more adequate guide to the nature of the contents than is the title. The volume is divided into four parts, called respectively the methodological, sociological, psychological and political science sections.

At the outset, Brown suggests that social psychology must deal with four types of problems: (1) the reactions of an individual to another individual, (2) an individual reacting to a group, (3) the reaction of a group of individuals to a single individual, (4) groups of individuals reacting to other groups. Believing that the organismic philosophy is the only acceptable one in science, Brown proposes to employ it in building a methodology with which to approach these problems. He emphasizes that organismic philosophy holds that the whole is more than the sum of its parts. "We mean that nature exists in the form of structured fields, and the structure or arrangement or pattern determines the activity of the individual part." He assures us that the inductive method has failed in science and that the "hypothetico-deductive" method is the "true" scientific method.

In scientific procedure, he says, one should begin by describing phenomena in terms of one's own experience (language of data). These experiences are designated phenotypes. One should then assume that underlying the phenomena are dynamic situations (genotypes), which may be described in a language of constructs. Constructs are conceptual or theoretical schemata, such as the gravitational field or Euclidean space, which account for the characteristics of the experiential world. In any science, "explanation does nothing more than order the language of data to theoretical constructs in such a way that predictions can be made." Brown recommends that mathematical constructs be used whenever possible, since "the constructs of mathematics are those to which competent observers most readily give universal assent."

The construct which Brown proposes for social psychology is the social field, comparable in certain respects to the fields of mathematics and physics. In this imaginary field are organisms (represented by points) whose behavior is directed toward a goal or goals. The force behind the behavior has a magnitude, but since in most cases the magnitude cannot be measured, it is designated by an index number. From topology, " a science which deals with the positional relationships which are independent of distance and direction," are borrowed concepts dealing with the non-metrical aspects of space. For psychological purposes, topology is redefined as " the science which investigates the ' belongingness ' of spatial regions, and their connectivity with other regions." The various aspects of the social field are described by such terms as region, boundary, membership-character, barrier, permeability, fluidity, vector, and locomotion. Here one is reminded of the social planes and currents of E. A. Ross.

As the reviewer finds it impossible to summarize briefly Brown's brilliant and scholarly presentation of the argument, he offers the following paraphrase: The old biological analogies which treated society as an organism were too limited and moreover, they were pre-Galilean. A better supposition is that although the whole universe is not necessarily an organism, it operates on the same principles as an organism (*i.e.* it may be described in terms of organismic laws). Let us then treat the phenomena of our experience as structured wholes which occur within nature, bearing in mind always their relationship to the universe as a whole. But to be scientific about it, we must also describe our data in terms of their underlying dynamic situations. Mechanism and vitalism are not suitable principles on which to base our descriptions of these dynamic situations, but there remains the possibility of using mathematical description, as the physicists do. (After all, mathematics *is* the language of science.) Let us adopt the analogy of the *field,* which has proved so useful in physics. We can retain our organismic laws by attributing to the social field such characteristics as " membership-character " and by modifying the mathematician's concept of space wherever necessary. Our scientific procedure will then be to reduce the phenomena of experience to statements of their spatial relationships. [" When we say that the construct of the psychological field is a spatial construct, spatial must be understood as pertaining to all *possible logical constructs about positional relationships.*" (Author's italics.)] When we know what these spatial relationships are, we can re-translate them into terms of our experience (language of

data). Thus we have achieved prediction, which is the goal of science.

Brown avoids stating that the social groups and the situations with which he deals are to be considered as *wholes*. However, the *fields* which he uses to represent the "underlying dynamic situations" frequently have the characteristics of wholes and individuals derive their nature and behavior from membership-character in the group.

As a sample of field-theoretical analysis, Brown gives a phenotypic description of a crowd becoming a mob, which he then translates into the language of field-theory. This is also known as "ordering the language of data to theoretical constructs." Field theory "is the only possible theory which allows us to explain the behavioral differences between the crowd and the mob without recourse to dichotomies or the introduction of unobservable causal factors." Having read this passage, many psychologists will return with satisfaction to such treatments of the crowd as F. H. Allport gives in his *Social Psychology*. Yet Brown's adverse criticism of the conflicting data from recent experiments on individuals working in groups is pertinent.

Brown proposes in the sociological section "to start with a field-theoretical discussion of the behavior of the largest groups and proceed by the process of abstraction to consider smaller groups until we arrive at primary groups." The emphasis is mainly on national, church, social class, minor group, and family membership-character, especially as these are related to war and the class struggle. In this section the numerous diagrams representing various field structures are excellent illustrations of the methodology and techniques employed in field-theoretical social psychology.

The psychological section is devoted largely to personality. Brown defines personality as "the pattern of the individual personality traits, which in themselves are largely determined by the structure of the field underlying the individual's membership-characters." The individual, who has been regarded previously as a point in a field, is seen to have a structure of his own, represented topologically by layers, the deepest of which is self. Hereditary traits are "traits determined in the *embryological field* [*embryological field*=zygote (embryo) in the surrounding intra-uterine environment]." (Author's italics.) Most of the traits, however, are determined in the social field. Among the personality traits he mentions are physique, intelligence (which may be increased as much as 40% by psychoanalytical treatment), goals, and environmental strivings. Brown leans strongly

on the Freudian explanation of personality genesis, expressed in terms of field theory. He insists that social psychology as an applied science must stress society rather than the individual, since "if society falls into chaos, the individual must fall with it." He does not add, "and *vice versa*."

In the political science section, the diagrams are no longer used, though the author presents some index numbers representing degrees of freedom of social and economic locomotion. He does not pretend to discuss political science as such, but rather "the influence of economic structure and the nature of the state on social psychology." The state is "the paraphernalia by which certain regions of the social field impose barriers and boundaries on other regions of the social field." Laws, mores and public opinion are barriers in the social field. Brown is admittedly anti-Fascist in his discussion of the Fascist state on the ground that Fascism is unscientific. His point of view in discussing the liberal democracy and the communist dictatorship is largely Marxian. This he justifies on two grounds: (1) Marxian theory more nearly fulfills the criteria for a field theory than does any other theory; (2) Marx more adequately predicted the present world situation than has any other social philosopher.

At first the author is fairly consistent in giving phenotypical descriptions of his data, followed by contrasting genotypical descriptions. As he progresses he becomes so enthusiastic about the language of constructs that he mixes it with the language of experience. For the novice, it becomes difficult to distinguish one from the other. The following sentences, taken from their context in a treatment of the church as a subregion of the national field, illustrate this:

Phenotypical Description	*Genotypical Description*
1. "When nations go to war or undergo severe industrial panics or revolutions so that the national social field undergoes radical restructurization, the church is invariably affected also."	1. "In general we may say that when the modern nation goes to war the potency of national membership-character is enhanced far more than that of the church. The church must either accept the war, or allow its members to lose national membership-character, or lose its members."
2. "In modern life, for most people, when a locomotion is blocked by both state and church barriers, those imposed by the church are more permeable. Except to a few individuals the potency of national membership-character was much greater than that of the church."	2. "The potency of national membership-character is greater than that of religious membership-character and the degree of freedom of social locomotion less for the national than for the religious region."

Brown's enthusiasm also leads him to forget that the social field was to be merely a construct of the imagination to which phenomena might be ordered. He endows the field with objective reality. The statements, " One of our chief conclusions in this work will be that membership-character in a social field is the chief single determiner of the individual's aims and ambitions, his fears and hopes," and " Human nature does not exist independently of the social field " are typical examples.

One can hardly deny that spatial relationships must be taken into account in studying social behavior. Brown's diagrams representing social fields are useful devices for dealing practically with these relationships, especially for persons whose imagery is predominantly visual. But diagrams of this sort were in use long before field theory was introduced to psychology. Take for example the diagrams used by football coaches in developing plays and teaching them to players. The " language of constructs " applying to football has become so familiar to educated Americans that they seldom employ " phenotypical " language in describing football games. One must admit that the methodology and techniques of the coaches are workable and it would be unfair to deny the same merit to those of the social field theorists. One must remember, however, that spatial relationships are of utmost importance in football. The coach's diagrams correspond to an actual field, and when the coach " orders " his men to the field, he does it literally and not figuratively. Brown's methodology assumes the same relative importance of spatial relationships in all situations involving two or more individuals. If spatial relationships were the only aspects of any importance in social behavior, his methodology would be thoroughly sound. As it is, his explanations and his predictions are valid only in so far as the spatial aspects of social situations provide a basis for explanation and prediction. Translating other aspects of behavior, such as beliefs, emotion and folk-ways, into spatial analogies will not improve the quality of generalizations based on spatial concepts. Brown unwittingly reduces this practice to absurdity when he represents honesty (one of the *mores*) as a *barrier* in the social field.

Although Brown does demonstrate that social behavior may be described, in part at least, in a technical language of spatial and dynamic analogies, he seldom makes predictions on the basis of these genotypical descriptions. The predictions he makes could have been made without invoking the cumbersome device of the social field. Some of his most important conclusions are those in which he agrees

with men whose predictions were made without benefit of his techniques. Often the author's most penetrating observations of social behavior are made on the basis of *historical* method, which is quite distinct from the field-theoretical approach.

Though the book is an excellent presentation of the methodology of the social field, it has failed to convince the reviewer that greater progress will be made in social psychology by "ordering social behavior to spatial constructs" than by describing it in terms of physiology and neurology. Brown's method has the disadvantage of leading one away from any attempt to relate social psychology to other fields of science. Again, charging behavior to "membership-character in the group" seems to offer few advantages over the genetic method of studying the development of social (particularly *institutional*) attitudes. This reviewer finds it easier to treat an organism as a bounded whole than to admit that a group of persons is a whole, the individuals deriving their behavior from membership-character in the group.

Until a more satisfactory methodology is developed, psychologists may well consider social psychology to be a branch of psychology, rather than a separate discipline. They would do well, however, to emulate Brown's aggressive attack on current social issues.

ARTHUR JENNESS.

Harvard University.

PLANT, JAMES S., *Personality and the Cultural Pattern.* New York: The Commonwealth Fund, 1937. Pp. x+432.

This volume is "an outgrowth and expansion" of lectures given at Yale in 1933–1934 before Professor Sapir's seminar on "The Impact of Culture on Personality." It is not a textbook. Rather it presents an attempt to formulate a philosophy for the clinic. Like all philosophies it raises more questions than it even tentatively answers. It possesses no systematic documentation; only a few references are given to articles and books which relate quite directly to the author's theses.

Although a medical man himself (Director of the Juvenile Clinic of Essex County, New Jersey) Plant does not have the usual psychiatric biases. His attitudes are much more those of the sociologist and the anthropologist. He does, in fact, chastise the psychiatrist for trying to take the patient into the clinic and so out of his normal cultural surroundings. The psychiatric social worker, the parent, the educator, and the psychoanalyst should be, but commonly are

not, more culturally minded. All those who have dealt with the child in the past have made his training too God-centered, too family-centered, or too nation-centered. They should have made it more child-centered, *i.e.* more individual-centered.

Part I of this book deals with a "new approach to the understanding of the personality." According to Plant the most workable theory of personality is that of *psycho-osmosis,* the idea that there is a highly permeable wall about the personality through which flow currents of environmental influence. Plant sees this notion as quite similar to H. Piéron's "envelope" and to the concept of "valence" in the meaning of L. K. Frank and K. Lewin. The method of personality study proposed is that of the *casual breakdown* which exists in an individual "who is presenting a short and dramatic dislocation of his usual relationships with any given social institution or social pattern."

In Part II are offered "examples of the interaction of the personality and the environment, drawn from studies of the casual breakdown." To the psychologist the most interesting chapter is that on the structure of the personality. The inherent elements of personality are tentatively listed as alertness (not IQ), complexity, pliability, temperament (extraversion-introversion), and cadence (rate of maturation). The acquired mental attitudes are placed into three categories: toward security, about reality, and about authority.

In Part III there are discussed: the road to an individual-centered culture, the family, the school, recreation, law and order, social work, medicine, the church, industry, and education for change. Each topic is treated under the subheadings "the search for meanings," "current assets and liabilities," and "the outlook for change."

Throughout most of his book Plant urges a fairly realistic approach to all clinic problems. At times, however, he appears to the reviewer to be rather unrealistic. For example, Plant advises educators to accept as a normal-school applicant only that girl who shows "a healthy and satisfying play relationship with other young people of her own age." (Excellent as this might be, what school board could properly do the necessary judging?) The applicant also should have had an "adequate" (whatever that might mean!) family experience in childhood. Again, Plant regards most delinquent acts as "normal reactions of normal people to abnormal conditions." At least such an attitude probably makes for excellent rapport with the patient! Plant closes his book with the statement that the child must be prepared for rapid changes in culture. This

preparation can best be managed by the setting up of partial goals which must frequently be examined and occasionally be alte

Whether or not the reader agrees with all he reads in this book he must regard its author as a most unusual physician to whom he can listen with considerable gain.

PAUL R. FARNSWORTH.

Stanford University.

SCHMIDT, EMERSON P. (Editor), *Man and Society: A Substantive Introduction to the Social Sciences.* New York: Prentice-Hall, Inc., 1937. Pp. xv+805.

In many colleges, committees have been appointed to plan compulsory survey courses in the social sciences for the freshman or sophomore years. To psychologists who have been forced to serve on such committees (and the reviewer has so served) this review of *Man and Society* is directed.

All but two of the contributors to the text are currently on the staff of the University of Minnesota and have presumably been close to courses of this nature. E. D. Monachesi opens the text with a very readable chapter on "Sociology and Culture." His offering is followed by J. O. Hertzler's (Nebraska) chapter on "Social Institutions" and by W. D. Wallis' on "Social Anthropology." Both sections are clearly written. The section on "Social Psychology" by H. Blumer (Chicago) should be of interest to the theoretically minded social scientist, but impresses the reviewer as being totally unsuited for the unsophisticated student. In the main it limits its discussion to a defense of the theories of the symbolic interactionists against those of a 'straw man'—a so-called stimulus-response group. H. P. Longstaff's essay on "Psychology and Some of Its Applications" seems splendidly pitched for its prospective reader. The reviewer feels, however, that a more currently believed in treatment of the nature-nurture issue would have been in order. (Longstaff bases a large part of his 'proof' that intelligence is inherited on studies of racial differences and on data as hopelessly inconclusive as those from the pioneer Kallikak study.)

Clean-cut chapters on "Modern Criminology" by G. B. Vold, on "History" by L. D. Steefel, "Human Geography" by R. A. Hartshorne, "Elements of Political Science" by E. M. Kirkpatrick, and "Popular Participation in Government" and "The Machinery of Government" by J. R. Starr follow in the order mentioned. In the reviewer's opinion the first two of the next three chapters on

economic subjects by the editor, E. P. Schmidt, are not so satisfactory. No matter how well treated (and these are nicely handled) the topics of "The Economies of Price" and "The Distribution of Income" cannot be clearly presented to a freshman in sixty-nine pages. On the other hand the subject of "Economic Security" lends itself to interesting presentation.

Of the final two chapters the one on "Causal Relationships and Their Measurement" by R. L. Kozelka is surprisingly clear for such a brief essay on statistics. The other, "Social Valuation" by M. J. Shaw, is philosophical in character. The reviewer does not believe that it will prove very enlightening to the beginning student.

The student for whom *Man and Society* was written will presumably devote nine or ten quarter hours to it. Will he achieve more from this than from two five-hour elementary courses in the social sciences? While granting every man a right to his own pet educational philosophy the reviewer casts his vote for the latter option.

PAUL R. FARNSWORTH.

Stanford University.

DURET, R., *Les aspects de l'image visuelle.* Paris: Boivin et Cie, 1936. Pp. vi+148.

In the four essays contained in this book, the author attacks experimental psychology for the sterility of its researches on imagery, develops his own views on the nature of imagery, and considers the relation between imagery and art.

The experimental psychologist, typified by Piéron, has approached the study of imagery under the handicap of two prejudices. These are the biological attitude and the belief that the image is an internal copy of an external object. The first prejudice has led to the errors of atomism, epiphenomenalism, the verbal confusion of image with brain cell, and the failure to distinguish between memory and habit. The second prejudice has predetermined the results of the experimental studies of localization of the image to show localization in or near the head. The author's own study of localization shows that an object is imagined to occupy its usual position or a position in which its outstanding characteristics are made apparent.

Since the object is imagined as being in a particular perspective, the image is specific. However, the usefulness of memory depends on knowledge of the whole object. This knowledge can be given only by thoughts of the movements required to develop the specific perspective into the series of perspectives required to characterize

the whole object. Thus, the thought of an object consists not only in a specific representation of it, but, also, in a motor orientation toward it and in thoughts of other movements with respect to it.

In the third essay the author attempts to discover the characteristics of imagery by an analysis of pattern vision as revealed in works of art. The analysis confirms the belief that the image is specific although the thought of an object includes all its characteristics. Schools of painting tend to oscillate between the two extremes of representation of part or whole. In life we are able to choose, through our own movement, the perspective that is most useful or aesthetic. Therefore the painter should portray a specific perception, but, at the same time, should suggest the movements needed to correct or enlarge the perception.

In conclusion the author reiterates that the image is not an internal object. It is a mode of conduct relative to some object. This relation between individual and object has a number of aspects, of which two, the representative and the instrumental, are fundamental. The image resembles both percept and concept.

The book contains many stimulating passages. This reviewer, however, is more impressed with the faults of the book than with the virtues. The use of leading questions and casual introspections in the author's studies is surprising in view of his strenuous attacks on the methods of previous investigators. The book, due perhaps to its organization into separate essays, suffers from constant repetition of statements. Recent literature, other than the French, is seldom considered. Gestalt psychology is not mentioned in a lengthy discussion of atomism. Recent experimental investigations of memory, published in English, receive no attention. Finally, although the author constantly repeats that the image is a relation between individual and object, he nowhere adequately defines the nature of the relationship.

CHARLES W. BRAY.

Princeton University.

TROW, W. C., *Introduction to Educational Psychology.* Boston: Houghton Mifflin Company, 1937. Pp. xiv+417.

Among the outstanding characteristics of this book are its unusual readability and teachability. Its readability is achieved through the simple straight-forward style of the author and his selection and presentation of significant material, and its teachability through the plan of organization, which includes a brief paragraph of introduction,

the division of the chapters into sections and subheads, a summary paragraph, and questions and selected references at the end of each chapter. In addition to such learning and teaching helps, there are a glossary and a supplementary book list at the end of the book. The book list is made up of two groups: Group I, which contains the more professional writings, and Group II, which contains the more popular volumes on educational psychology. The inclusion of the photographs of twelve eminent psychologists adds a bit of interest for the beginning student.

The book presupposes no previous work in psychology. The reviewer, having been accustomed to that organization of psychology courses in which general psychology is a prerequisite to educational psychology, wondered whether this book would find an immediate place in colleges and universities or whether it would have to wait for adoptions upon modification of existing curricula. In order to make an attempt to find the answer to that question, the reviewer scanned the offerings in education and psychology in a random sample of colleges and universities and found that the ratio was four to one in favor of the requirement of a general course, usually three hours, as a prerequisite to educational psychology. The schools offering educational psychology without a prerequisite were usually those with a large school of education having a large enough enrollment to offer a separate beginning course for students majoring in education. The author has done an interesting piece of work in showing what can be done in the way of combining the traditional general course with the beginning work in educational psychology. It may be that this book indicates one method of reducing the amount of duplication in courses, concerning which students in education often complain so bitterly.

Because no previous work in psychology is assumed, one finds material on sensation, perception, and the physiological basis of behavior, interspersed among chapters on individual needs, personality adjustment, intelligence, measurement, and transfer and the school subjects. The author has done amazingly well in avoiding both the Scylla of psychology and the Charybdis of education. The book presents many of the important experiments in both psychology and education and points out their application to individual problems and to the science of education.

In his chapter on transfer and the school subjects, the author clearly makes the point that the ability to make use of general principles taught in the classroom does not come automatically to the

pupil, but must be developed by the teacher. He remembers this principle all the way through his book and is careful to indicate how the material presented may be used by the prospective teacher and to show its meaning in terms of the general school program. For example, under habit and skill there is a discussion of the part of the teacher and of the school in correcting speech defects and improving speech habits.

The beginning student will find the same material presented to him under different headings. For example, the problem of the influence of heredity on individual development is discussed in the chapter on childhood and youth as well as in the one on individual differences. The term "eupedic program," which the author prefers to the "rather vague phrase character education," is defined and discussed in the chapter on personality adjustments and again in the chapter on childhood and youth.

Of course, as the author states in his introduction, the author of a book of this kind must of necessity choose that material which seems most significant to him and neglect much that another writer in the same field might have included. The present reviewer regrets that only one page is devoted to educational guidance and that so little attention is given to the importance of guidance programs in the schools. The important educational question of the formation and modification of attitudes also receives rather scant attention. On the whole, the book is an excellent presentation of much of the significant material in the field of educational psychology and should prove a very useful text for introductory courses.

DOROTHY C. STRATTON.

Purdue University.

FISHER, V. E., *Auto-Correctivism, the Psychology of Nervousness.* Caldwell, Idaho: The Caxton Printers, 1937. Pp. 337.

Dr. Fisher has attempted a comprehensive theory of the neuroses based upon the division of all human motives into two types, one a group of racial and selfless motives, the other a group of egoistic and selfish motives. Between these two groups there exists an inherent antagonism. There also exists an inherent regulatory principle which maintains a quantitative balance between them by thwarting the overextension of either. This regulatory principle is designated as the auto-corrective motive. Whenever either type of motive tends to become overdeveloped at the expense of the other, auto-corrective

action automatically results in behavior designed to thwart this overdevelopment. This corrective action explains neurotic behavior. The neurosis is purposive in nature and serves the auto-corrective principle as a means of maintaining balance in the motivational system. The neurotic symptom is directed toward the thwarting of the overdevelopment of either type of motive by inhibiting its progress and turning the energy of the individual into the opposite channels. Thus a teacher who tends to make too strong a selfless identification with her pupils may find herself developing neurotic symptoms as an auto-corrective measure to hinder the further development of such identification and to turn her attention back toward her own self with a reinforcement of selfish, ego motives.

The idea is an interesting one, but unfortunately it has not been carefully worked out. At no point is the argument backed up by reference to experimental findings or sufficient clinical data to make the case convincing. Instead, it is allowed to rest largely upon the authoritarian fiat of the author. That there is much in the book that will not antagonize, and may even please, the mental hygienist is not so much evidence of the force of Dr. Fisher's argument as it is of the gradual development of a large body of commonly accepted concepts in the field.

WILLIAM A. HUNT.

N. Y. State Psychiatric Institute.

BROWN, HENRY COLLINS, *A Mind Mislaid.* New York: E. P. Dutton and Company, 1937. Pp. 219.

With no burning desire to reform institutional life, with no predilection for some type of cure for mental disorders, with good humor and homely philosophy, Henry Collins Brown tells the story of his three years' residence at the Bloomingdale Hospital. He tries to "show the world that a nervous breakdown is not necessarily the end of all things, . . . that there is absolutely nothing mysterious or supernatural about a breakdown of this kind. . . . Furthermore—and this is the sad part of it, mates—you are where you are largely as the result of some damn foolishness on your own part."

He gives little credit to the work of any particular type of treatment, except to Dr. Bleuler for giving him the assurance, early in his illness, that he would get well. He sounds warnings against the "vast army of charlatans professing to be psychiatrists" who have all the trappings of the reputable physician but whose only purpose

is to exploit the unwary. He laughs at writers in the field of psychiatry who do not "know the meaning of the words they employ and who confuse the reader and bewilder the public."

He attributes his difficulty to disappointment in losing his job as manager of a museum he had dreamed about and planned for years. He believes his cure came when he realized that he still had something for which to live. The main part of the book is concerned, however, with a discussion of the behavior of other patients and of the conduct of the hospital, rather than with a discussion of his own symptoms, his own struggles to get well, or his feelings about the hospital.

He does a very creditable job in breaking down the barrier that has existed between the so-called normal man and the one who is mentally ill. He laughs at the ridiculous things that men do, but he does it in an objective manner, which lacks that element of condescension so often found in such jokes.

The book should help create a saner attitude on the part of those who believe they are mentally sound toward those who are being treated in mental institutions. As the author puts it: "I often think that a two weeks' sojourn in a mental hospital, at an early period in life, would be one of the best things that could happen to all of us."

JOHN J. B. MORGAN.

Northwestern University.

KUGELMASS, I. NEWTON, *Growing Superior Children.* New York: D. Appleton-Century Company, 1935. Pp. xvi+568.

The title of this book may lead one to suppose it to be a treatise on the development of superior deviates, but it is not. It is about superior care of children, not about superior children. It undertakes to present current theory and practice in child care, physical, mental, and moral, for children in general.

This is to say that the book is very eclectic, and no one reviewer can be expected to comment with authority upon all sections of it. The topics range from the Aschheim-Zondek test for pregnancy to social adjustment; from special poultice procedures to IQ; from exercises for weak feet to religious education. This range of topics is too vast for adequate, authoritative or systematic treatment by one author, even in 568 pages. The general impression is of hundreds of complex matters touched upon with running comment, no full discussion being given of any of them.

The style of the author is that of the physician, addressing *ex*

cathedra (or more properly, *ex hospice*) those who would have his advice. It tells only what the adviser knows or has decided about the matter under consideration, without telling the source of the knowledge, or referring to the literature (possibly dissenting) of the subject. Being thus undocumented, the book is not adapted to the uses of students of child development. This is not a fault, however, as it was not intended for students as such, but for parents.

As a book for parents, it is of value for those having a vocabulary equal to it, with the help of the glossary of terms which is appended. Generally speaking, educated parents will find here an outline of the topics ordinarily considered in texts on child development in its various branches today, but not more than an outline.

Of course, not any one author (any more than any one reviewer) can be truly expert and sound in all the areas of development touched upon by Dr. Kugelmass. It is, therefore, neither surprising nor disappointing that we find errors of statement in what he has written. In the reviewer's particular area of competence, notice may be taken of the following erroneous statements: "Most children have latent qualities of superiority in one or more special fields" (p. 400); "Children with an IQ below 60 rarely reach the fourth school grade, those below 80 rarely graduate from the elementary school, and those below 100 rarely complete high school" (p. 399); and "It is impossible to group children in the elementary school according to individual ability" (p. 393). Perhaps what the author means is that children with the IQ's specified on p. 399, *should* not graduate, but the fact remains that they do. As for the statement quoted from p. 393, the author himself seems later to realize that it is erroneous, for on p. 403 he says: "The first requirement is to accommodate the gifted child in a special class for each age. . . . Every school system should provide special classes for the gifted one per cent of the school population."

The present reviewer is not prepared to comment upon the soundness of sections on scientific feeding, digestive disorders, infectious diseases, religious education, and the like.

To sum up, the book contains general advice on the rearing of children, offered by a physician (pediatrician to various hospitals), who speaks on his own authority, covering a multitude of fields, without documentation. The descriptive adjective that comes most insistently to mind after reading the book is "miscellaneous."

LETA S. HOLLINGWORTH.

Teachers College, Columbia University.

VALLET, A., *Mes conférences: Sur les guérisons miraculeuses de Lourdes.* Paris: Pierre Téqui, 82 Rue Bonaparte, 1937. Pp. xiii+266.

Mes Conférences presents a number of addresses concerning the "Miraculous cures of Lourdes." The book is a result of ten years of study of cures which have been presented to the Bureau Des Constations Médicales. It involves a study at Lourdes of the cases, their particular symptoms and distinctive characteristics, in order to define the disease. Further information is supplied by the physicians who have treated the cases before their arrival at Lourdes. Unfortunately this coöperation with the Bureau is from a medical association in sympathy with the work of the Bureau. It is not composed of such physicians as one would find on the faculties of the French medical schools. The third phase of the study, by the Bureau, is to determine the character of the cure.

Dr. Vallet presents his material as an argument defending the miraculous features of the cures. He first calls attention to the fact that the waters of Lourdes have no peculiar chemical or radio-active characteristics. A second argument is that the cure occurs instantaneously and the third is the absence of a period of convalescence.

To the frequent explanation that the cures result from suggestion Dr. Vallet insists that some of his cases are obviously due to organic disorders which could not be reached by suggestion. To establish this contention and to prove that the thoughts of the patients are not necessary to achieve a miraculous cure he cites at length the case of Mlle. Guinot. This patient was suffering from tuberculosis that showed itself in a number of ways. When she arrived at Lourdes she was unable to determine when and how the cure occurred; she was only aware of the fact that her condition improved on a given day. Dr. Vallet states that, by her education and her personal convictions as well as by her religious practices, she was an unbeliever and that in her case a religious faith could not be the explanation of the cure.

Many pages are devoted to a polemic against scientific incredulity. The interesting features are accounts of cures, but these do not carry conviction owing to the author's avowed intention to prove them of miraculous character. Should a bureau be established that gave an adequate case-history of the patient and an impartial account of the cure we might obtain some data that would enable us to classify these cures and obtain some dependable scientific material.

H. C. MCCOMAS.

Johns Hopkins University.

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NOTES AND NEWS

THE meetings of Section I (Psychology) of the A.A.A.S. will be held in Indianapolis at the Columbia Club, from Monday, December 27, to Wednesday, December 29, 1937. In addition to the regular program of submitted papers, there will be a special symposium by psychologists and physiologists under the chairmanship of Dr. Calvin P. Stone of Stanford University, on *The Endocrines as Related to Behavior.* There will also be a joint banquet with Section Q (Education) at which the vice-presidents of the sections will give their vice-presidential addresses.

Abstracts of three hundred words or less, to be submitted in duplicate by members and fellows of Section I should reach the secretary, Leonard Carmichael, Department of Psychology, University of Rochester, Rochester, New York, by November 6, 1937. Papers to be presented before the meetings must not require longer than fifteen minutes for presentation.

DR. DOUGLAS MCGREGOR of Harvard University has been appointed instructor in psychology at the Massachusetts Institute of Technology, and he thus becomes the first full-time psychologist appointed at that institution.

Dr. Arthur F. Jenness, while on leave from the University of Nebraska, has been appointed lecturer in social psychology at Harvard University to give Dr. McGregor's work in 1937–1938.

Wilbur S. Gregory, who is completing his work for the doctorate under Dr. F. H. Allport at Syracuse, will take over Dr. Jenness' work as special adviser to freshmen in the College of Arts and Sciences at the University of Nebraska, and will offer courses in general and social psychology.

DR. HENRY A. MURRAY has been appointed associate professor of abnormal and dynamic psychology at Harvard University, and has been given two years' leave of absence in order that he may complete his writing up of the recent research of the Harvard Psychological Clinic.

Dr. Roger G. Barker, General Education Board Fellow with Professor Lewin at the University of Iowa, has been appointed

instructor in child psychology at the Harvard Psychological Clinic during the first year of Dr. Murray's absence.

Dr. S. S. Stevens has been appointed faculty instructor at Harvard University and takes over the work of Dr. C. C. Pratt, who goes to Rutgers University as professor of psychology.

Dr. Carlyle F. Jacobsen, assistant professor of comparative psychobiology in the Yale School of Medicine, has been appointed assistant professor of psychology in the department of psychiatry of the Cornell Medical College.

At the recent Centenary of the University of Athens, Greece, the degree of *Docteur Honoris causa* was conferred upon Dr. Carl Murchison, formerly professor of psychology at Clark University and now editor of the various psychological journals published by The Journal Press at Provincetown, Massachusetts.

At the end of the present academic year, Dr. Lightner Witmer retired as professor of psychology and director of the Psychological Laboratory and Clinic at the University of Pennsylvania. At commencement, the University of Pennsylvania awarded him the degree of Doctor of Laws in recognition of his contributions to science and human welfare and especially for his founding of the first Psychological Clinic at the University of Pennsylvania in 1896. That evening a dinner was given Dr. Witmer by a group of his former students who had obtained the Master's and Doctor's degrees under him in psychology, with an attendance of approximately 200.

Dr. Witmer will continue as director of the Witmer School at Devon, Pennsylvania. The Witmer School is a residence school for not more than fifteen children, a day school for not more than ten children, and an observation school for the psychological examination of children who may need corrective or educational guidance at home or at school.

Dr. Richard E. P. Youtz, Dr. S. D. S. Spragg, and Dr. Ray Simpson have been appointed instructors in psychology at Barnard College, Columbia University. Donald MacMurray is to be assistant in psychology.

Dr. John Volkmann of Harvard University and Dr. Dwight W. Chapman of the Psychopathic Clinic, Recorders Court, Detroit, have been appointed instructors at Columbia University.

Dr. William D. Turner has been appointed research psychologist at the Institute of the Pennsylvania Hospital, Philadelphia,

Pennsylvania, where he will conduct, among other projects, an investigation of psychological changes in schizophrenic patients receiving the insulin-shock treatment.

A research study to determine the psychological effects of so-called "sympathomimetic" drugs is in process of organization at the Institute of the Pennsylvania Hospital. A grant of funds has been made to cover the project during the first year. The experimentation and clinical studies will be conducted principally by William D. Turner and George P. Carl, both members of the psychological staff of the Institute, while Drs. Earl D. Bond, William L. Long, and Edward M. Westburgh will act as a committee of consultants and control the administrative features of the study.

Sociometry: A Journal of Inter-Personal Relations made its first appearance with the July–October number. Gardner Murphy, Columbia University, is editor, and Eugene Horowitz, Albany, New York, managing editor. Contributing editors are: Gordon W. Allport, Harvard University; Hadley Cantril, Princeton University; Helen Jennings, New York State Training Schools for Girls; William H. Kilpatrick, Columbia University; Nolan D. C. Lewis, Columbia University; Eduard C. Lindeman, New York School of Social Work; Robert S. Lynd, Columbia University; Theodore M. Newcomb, Bennington College; and Gertrude Franchot Tone, Beacon Hill.

Sociometry will be a quarterly publication, each annual volume consisting of at least 400 pages. The major share of the contents will consist of contributions from sociometry and a psychiatric approach to human interrelations, but the journal will not be limited to such material. Contributions will be drawn from the fields of anthropology, child development, education, psychiatry, social psychology, and sociology, and editorials, book reviews, and general announcements will be included. All communications should be addressed to the managing editor, Post Office Box J, Beacon, New York.

THE Second Conference on Methods in Philosophy and the Sciences will be held on Sunday, November 28, 1937, at the New School for Social Research, New York City. The meeting will be in the nature of a symposium on the concept of law in the sciences, the morning session being devoted to brief papers on and discussion of the topic in relation to the physical sciences, and the

afternoon to a similar session on the concept of law in the social sciences. Those interested in attending the conference should communicate with the secretary, Gail Kennedy, Amherst College, Amherst, Massachusetts.

THE Second National Conference on Educational Broadcasting will be held at the Drake Hotel in Chicago, November 29, 30, and December 1, 1937. Dr. George F. Zook, president of the American Council of Education, will again act as conference chairman, and Dr. C. S. Marsh, vice-president of the council, 744 Jackson Place, Washington, D. C., is the executive secretary.

THE Fifteenth Annual Meeting of the Academy of Physical Medicine will be held at the Hotel Walton, Philadelphia, Pennsylvania, October 19, 20, and 21, 1937. The Academy will present a scientific program based on reports of recent research and practice, and, in addition to the lectures, demonstration clinics will be held at the hospitals of the University of Pennsylvania, Jefferson Medical College, and Temple University. A copy of the program may be obtained from Dr. William D. McFee, 41 Bay State Road, Boston, Massachusetts.

THE Bureau of Human Heredity, affiliated with the International Human Heredity Committee, wishes to collect on as wide a scale as possible material dealing with human genetics. It would therefore like to receive from institutions and individuals well-authenticated data on the transmission of human traits. Pedigrees are particularly desired; twin studies and statistical researches are also relevant; and reprints of published work are acceptable. Further information may be obtained from the chairman, Professor R. Ruggles Gates, 115 Gower Street, London, W.C. 1.